—THE—

Therapeutic Gazette.

Whole Series, Vol. XIII. | DETROIT, MICH., PHILADELPHIA, PA., | March 15, 1889. | Third Series, Vol. V. No. 3.

CONTENTS.

Original Communications.

PAGE

Prophylactic Hygiene. By Professor Dujardin-Beaumetz..... 145
On the Separation of Motor and Trophic Nerves of Muscle. By Geo. T. Kemp, Ph.D..... 155
Catacausis Ebriosus (Spontaneous Combustion). By G. Archie Stockwell, M.D., F.Z.S..... 168
A Bacteriological Manipulating Chamber. By Samuel G. Dixon, M.D..... 174
Three Cases of Valvular Insufficiency. By W. G. Kemper, M.D..... 176

Leading Articles.

Phenacetine..... 178
Cocaine-Poisoning..... 179
Hemorrhage from Salicylic Acid..... 179
The Narcotic Properties of Eschscholtzia Californica..... 180
Iodide of Potassium in Syphilis..... 180

Reports on Therapeutic Progress.

PAGE

Alkaloids of the Areca-Nut..... 177
Primary Union after Excision of Tubercular Hip-Joints..... 181
A New Antidote for Morphine..... 184
Citrate of Caffeine in Eclampsia..... 184
A Hitherto Undescribed Cause of Delay in the Second Stage of Labor..... 185
The Treatment of Suffocative Attacks after Tracheotomy for Croup..... 186
The Therapeutic Action of Hyoscine... 187
The Treatment of Cardiac Dilatation... 187
A New Operative Treatment in Empyema..... 188
A Hitherto Unobserved Effect of the Salicylates..... 188
Radical Cure of Hernia..... 189
Contraindications for the Use of Antipyrin during the Menstrual Period.... 190
Intravascular Coagulation of the Blood as the Result of Different Drugs..... 190
A New Symptom of Pericarditis..... 190
Chronic Headache..... 191
The Antilithic Properties of Antipyrin.. 191

PAGE

Reviews..... 191

Correspondence.

Contributions to the Discussion on the Diagnostic Value of the Tolerance of the Iodides in Syphilis between J. William White, M.D., and H. C. Wood, M.D..... 195
Paris Letter..... 210

Notes and Queries.

A New Remedy for Cholera..... 212
Colds and Clothing..... 213
Ophthalmia Neonatorum..... 214
Pastes in Dermatology..... 214
Poisoning by Citrate of Caffeine..... 215
Kavaine..... 215
Albuminuria and Yellow Fever..... 215
Juniper-Berries as a Diuretic..... 215
The William F. Jenks Prize..... 215
Lesions of Carbolic-Acid Poisoning..... 216
Extract of Male Fern as a Tæniacide... 216
Thiol..... 216
Antipyrin in Obstetric Practice..... 216
Unalterable Iodide of Potassium Ointment..... 216

Original Communications.

PROPHYLACTIC HYGIENE.

LECTURE VII.—ON ISOLATION.*

BY PROFESSOR DUJARDIN-BEAUMETZ, PARIS, FRANCE.†

GENTLEMEN:—In two previous lectures we studied disinfectants and disinfection. We have still to examine another mode of prophylactic hygiene for opposing the propagation of infectious diseases,—namely, isolation,—and I propose in this lecture to consider the grave questions connected with this subject.

I shall leave to one side all that pertains to legislation, as in a special lecture devoted to this subject we shall have to examine with some thoroughness the various means which government has instituted for arresting the spread of infectious diseases. But what I can assure you in this connection is that if in our country we have laws and measures extremely severe in their application to animals affected with epizoötic diseases, this is not the case in reference to man, and in this particular we are almost completely disarmed.

You should be cognizant of this strange and almost incredible fact, that while we have the right on the public highway to

* Advance sheets of a lecture delivered in Cochin Hospital, and translated for this journal by E. P. Hurd, M.D.

† Member of the Academy of Medicine, Physician to Cochin Hospital, etc., Paris, France.

arrest a man who has become a nuisance or dangerous to his fellow-men by reason of arms which he carries, or the tumult which he occasions, or the outcries which he utters, we cannot prevent an individual affected with smallpox from perambulating the streets and spreading disease and death around him. You will see that in some foreign lands the inhabitants are much better off in this respect than we are, and you will understand the necessity we are under of adopting stringent laws.

Isolation must be examined under three aspects,—either it pertains to the individual alone, who must be separated from his family; or else it concerns a small number of individuals affected with the same disease, who demand collective seclusion; or it has reference to a large aggregation of people, who, on account of some epidemic, must be kept apart from the populations which surround them.

We shall examine successively individual isolation, collective isolation, and the isolation of cities or of entire peoples, the latter comprehending the sanitary *cordons** and the quarantines. But, before going further, we must first decide a knotty question,—to wit, For what diseases we ought to practise isolation?

We have before seen reason for the statement that, if every microbe-disease is not contagious, the converse is not true, and that we may with confidence lay down this law that all contagious diseases are microbe-diseases. For this reason, then, isolation should pertain to a great number of diseases, and embrace almost all the affections due to pathogenic microbes. But there are degrees of contagiousness, some diseases being eminently contagious, others less so, and the existence of these degrees has enabled us to establish divisions in relation to these maladies.

At the head of the eminently contagious diseases we must place diphtheria, and there is a general agreement in demanding rigorous isolation in this disease. Do not forget that diphtheria is one of the most fatal of affections, especially in Paris, and that each year the mortality in Paris from this disease exceeds two thousand, which corresponds to more than 1 death for every 1000 inhabitants. In 1882 the mortality attained the ratio of 186 per 100,000. Then come the eruptive fevers, and at their head is smallpox. In the case of measles and scarlet fever, the value of isolation is disputed by some authorities.

As for measles, Ollivier has endeavored to show the necessity of isolating children affected with this disease, by pointing out the ravages which measles make in children's hospitals. In fact, this affection, which is generally benign in children of good constitution, brings grave complications in those who are debilitated or predisposed to tuberculosis. There is, then, a real need of separating rubeolic patients in children's hospitals from other patients, but this isolation is much less urgent when you have to do with well-constituted children, and such as have inherited no tuberculous tendencies.

As for scarlet fever, there is a clear distinction to make between children and adults. In children, scarlatina is a very contagious affection. In 1886, at the Hôpital des Enfants Malades, there were admitted one hundred and thirty-two cases from outside, and eighteen cases subsequently occurred in the wards. At the Hôpital Trousseau, two hundred and two cases were admitted from without, and twenty-two cases were subsequently reported as occurring within. It is far from being a very contagious disease in adults, and the propagation of scarlet fever in our wards is so rare an occurrence that, since I have been physician to the hospitals, I have never seen a case of scarlet fever contracted in our wards from a scarlatinous patient, though, at our hospital Cochin, there is not a month that we do not admit such patients.

This is, then, to my mind, the least contagious of the eruptive fevers, and while recognizing the fact that in this disease in private practice individual isolation is obligatory, it is not so with hospital practice, where there are only adult patients, and I do not fear to take scarlet-fever patients into my wards.

By the side of these eruptive fevers we must place whooping-cough, which is also an affection eminently contagious. Whooping-cough is generally benign; does not take on a character of gravity, except in debilitated and delicate children. While in the city, deaths by whooping-cough are very rare, it is not so in some of the children's hospitals, as that of Enfants Malades, where, according to Ollivier, the mortality in 1886 was twenty-five per cent.

But where there has been less agreement and much contention is with reference to

* A line of soldiers to prevent communication with an infected district.—TR.

typhoid fever and tuberculosis; now that the bacillary nature of these two affections is well known, as well as their mode of contagion, it is no wonder that there is a disposition to isolate these affections.

In families, the isolation of typhoid patients can be easily carried out. Is it the same in our hospitals? Without denying that instances of typhoid fever may occur in our wards, this is, it must be confessed, a fact of exceeding rarity, and although some hospitals in foreign lands have instituted special wards for typhoid patients, I do not believe that isolation is here an absolute necessity from the point of view of good hospital management.

I must pronounce the same opinion with regard to tuberculosis. That it may be desirable in a certain measure to separate a husband or a wife suffering from a tuberculous disease from a partner who is healthy, and especially to forbid their cohabiting together, I freely admit; but that there is any necessity to found hospitals for the tuberculous, does not seem to me at all clear, for, if tuberculosis is really contagious, it requires, for this contagion to be effective, certain conditions which are very difficult of practical realization. If we undertake to isolate the tuberculous, we might about as well isolate all other diseases, pneumonia included, and hospital service under such conditions would no longer be possible.

To these diseases we must add the imported diseases, such as cholera, sudor Anglicus, yellow fever, the plague, diseases eminently contagious, and which urgently demand isolation.

Thus far I have only considered diseases purely medical. Surgery, in its turn, demands isolation for affections which are septic and eminently inoculable, and the same may be said of obstetrics, in the successful practice of which, thanks to the antiseptic methods and to the rigid isolation of puerperal affections, we have reduced to the lowest figure the mortality in our services of accouchement.

Having disposed of this first topic, let us now inquire how isolation is to be accomplished. I need say but little about individual isolation, as practised in the family. To place a patient in a large, well-ventilated chamber, to keep from him all persons who might contract the disease from which he is suffering, and to let only those come near him whose duty it is to wait upon him, is generally easy of execution, and all that is needed is the authority of the physician and the co-operation of the family to enforce perfect seclusion.

Individual isolation may be relative or absolute, and, with regard to the matter of relative isolation, permit me to insist upon that which tuberculosis requires. If I do not favor hospitals for the tuberculous, for reasons which I have given, I do heartily advocate the relative isolation of these patients. I believe that when in a household one of a married couple is tuberculous, it is dangerous for the two to occupy the same bed, or even the same bed-chamber. The same isolation is demanded for the offspring of tuberculous patients, who should not be allowed to sleep in the same room with their parents.

Such are the few remarks which I wished to make relative to individual isolation in the family. If there has been but little contention with regard to this kind of isolation, it is not so when we come to the matter of collective isolation. Under this head we have to examine questions of the gravest importance, and we will follow, if you please, the course adopted by Fauvel and Vallin in their able report on this question made to the International Congress of Hygiene in 1878, and which may be considered as a model of the kind.

We shall, then, have to study the three following points:

1. Does collective isolation augment the mortality of patients thus isolated?

2. Does this sort of isolation create centres of infection?

3. How ought this collective isolation to be practised?

On the first point, everybody is unanimous in recognizing that the aggregation into one place of individuals affected with the same contagious disease does not augment the mortality of these individuals, and I can in this regard give you figures which are demonstrative, especially with respect to smallpox and typhus.

Colin has shown us that during the terrible year of smallpox (1870–1871) the sanitary boards had succeeded in isolating all the smallpox patients of Paris in the Hospital of Bicêtre, which, during that year, received no less than seven thousand five hundred and seventy-eight persons sick with the disease. From October 12, 1870, to April 1, 1871, there were ten hundred and ninety-four deaths, giving a mortality of fourteen per cent., a mortality which is not greater than

that which is observed when no isolation is attempted.*

But England has given us the most important statistics in relation to this subject. This country has made great achievements in the matter of isolation, and, in particular, in respect to smallpox, and you will find in the remarkable study of Lutaud and Douglass Hogg on the insulated hospitals of England, very accurate and minute data on this subject.†

Consult the statistics furnished by the smallpox hospitals, and you will see that the mortality there is not greater than in cases and under circumstances where smallpox is not the subject of a collective isolation. For instance, glance over the statistics of Stockwell Hospital, one of the hospitals southwest of London, and you will see that the number of smallpox patients does not sensibly modify the mortality of that hospital.

STATISTICS OF THE STOCKWELL SMALLPOX HOSPITAL FROM 1871 TO 1876.

	Number of cases of smallpox.	Mortality.
1871.....	2288	18.6
18-2.....	601	18.9
1873.....	64	18.9
1874.....	59	8.5
1875.....	89	19.0
1876.....	800	20.2

In this connection, we may also appeal to the statistics pertaining to the isolation of individuals affected with typhus, and we must still go to England to obtain these statistics.

At the London Fever Hospital in 1862, out of eleven hundred and seven cases of typhus exanthematicus aggregated in the same wards, there were two hundred and thirty-two deaths, giving a mortality of 20.95 per cent. During the same period, in the other hospitals in London, three hundred and forty-three cases of the same disease were treated, with a mortality of eighty (23.3 per cent.). The same proportion was kept up the following years, for from 1862 to 1865 seven thousand four hundred and ninety-eight cases of typhus exanthematicus treated in the isolation wards of the London Fever Hospital furnished a mortality of 18.57 per cent. During the same period four hundred cases treated in the common wards gave a mortality of 21.15 per cent. The question is then settled, and we may affirm to-day that the grouping together in a common ward of patients affected with the same infectious malady does not augment the fatality of the disease.

Much more difficult to solve is the second question,—namely, If hospitals of isolation for infectious diseases create centres of infection, and thus become a nuisance to the neighborhood of those hospitals?

In Paris, since the institution of hospitals of isolation for smallpox in connection with the general hospitals, it has been stated that numerous cases of smallpox have occurred in the immediate vicinity of those hospitals; and in 1870 this was said to be the case with reference to the Hospital Laennec on Sèvres Street, where the existence of the smallpox pest-house caused this affection to spread in the surrounding streets. The same year Delpech made two reports in regard to this matter to the Council of Hygiene, and this question came up anew before the Council a little while ago in reference to the smallpox hospital situated at Aubervilliers.‡ In a report presented to this Council I made known the reality of the facts.§

Créquy, moreover, in a report to the Academy of Medicine, October, 1887, "On the Epidemic of Smallpox, which Broke Out among the Workmen of the Gas-Factory of Aubervilliers," communicated some facts, which seemed convincing with regard to the propagation of the disease from this hospital of Aubervilliers to the employés of the neighboring gas-factory.

But it is again from English sources that we obtain the most demonstrative statistics on this point. The Asylum hospital and the smallpox hospitals have been, like our own hospitals of isolation, accused of creating centres of infection for smallpox, and to decide this question a royal commission was appointed.

The reply of this commission was as follows: "It appears to us clearly established by the experience of five smallpox hospitals during these last six years that in their actual state these pest-houses are a cause of the propagation of smallpox in their vicinity. At the same time, the commission gives its opinion that direct communication and by individuals may have had a more important part

* Colin, "Smallpox from an Epidemic and Prophylactic Point of View." Paris, 1883.

† Lutaud and Douglass Hogg, "Étude sur les hôpitaux d'isolement en Angleterre." Paris, 1886.

‡ Delpech, "On the Epidemic of Smallpox observed in Paris from 1865 to 1870."

§ Dujardin-Beaumetz, "On the Cases of Smallpox observed in the Eighteenth and Nineteenth Districts of Paris, and in the Communes of Aubervilliers, of Pantin, and of Saint-Denis in 1887."

in the propagation of the disease than the air itself."

Hill, Tripp, and Powers, in their reports, have come to almost the same conclusions, and I must call your attention especially to the report of this latter physician, who studied the influence of Fulham Hospital on the propagation of smallpox.

This hospital, situated in the region west of London, and which has fifty beds for smallpox patients, gave Powers a good opportunity to investigate the matter and ascertain in different zones traced around the hospital the number of houses infected, and I cite *verbatim* the figures given by Vaillant in the report of Chautemps to the Municipal Council on the necessity of the isolation of infectious diseases.

Proceeding upon this basis, he traced around the hospital four circles, having, respectively, for radii a quarter of a mile (four hundred metres), a half mile (eight hundred metres), three-quarters of a mile (twelve hundred metres), and one mile (sixteen hundred metres). The entire area included in the larger circle he called the special zone. He then divided the length of time during which Fulham Hospital remained open for reception of patients into five epidemic periods, and he examined relatively to each period the proportion of infected houses in the central zone, and in each of the zones of a quarter of a mile radius which surrounded it. The result was that in each of the epidemic periods the disease propagated itself around the hospital in zones of decreasing intensity, the decrease being in the ratio of the distance from the hospital, as is shown by the above table, where, as in the entire report, the term "acute" designates all the cases which had not arrived at the period of convalescence.

All these figures show you that with respect to smallpox, hospitals of isolation create centres of infection, and are liable to propagate the disease, and this the more actively the nearer the houses are to the hospital.

It remains now to consider the second point,—namely, How is this propagation effected? Is it by the air or by individuals? This question seems now to be settled. If the air of smallpox wards may contain, as Brouardel has shown,* solid particles coming from the crusts of dried variolous pustules, these particles cease to be found in the air which surrounds the hospital. Thus it is that Colin fixes at one hundred metres the distance at which the air ceases to be contaminated. According to repeated inquiries and investigations, it appears to be demonstrated that it is by direct contact that the propagation of this disease is effected, and, as has been well said, the *rum-shop* has considerably more to do with this matter than the influence of the atmosphere.

With respect to the hospital of Aubervilliers, it is chiefly a drinking saloon called the *Château of Alouettes*, situated near the gate of the hospital, and where the persons accompanying smallpox patients are accustomed to obtain liquor, which has been the active propagator of smallpox in the vicinity of this hospital. Add that certain subor-

Cases of Acute Variola admitted to Fulham Hospital, and the Percentage of Smallpox occurring in the Houses of the Special Zone and its Different Subdivisions during Five Epidemic Periods.

Cases of acute smallpox admitted.	In epidemic periods since the opening of the hospital.	Percentage of infected houses in the special zone and its subdivisions.				
		In the totality of the special zone.	In the small interior circle of ¼ of a mile.	In the first exterior zone of ¼ to ½ mile.	In the second exterior zone of ½ to ¾ mile.	In the third exterior zone of ¾ to 1 mile.
327	March, 1877, to the end of 1877.	1.10	3.47	1.37	1.27	0.36
714	January, 1878, to September, 1878.	1.80	4.62	2.55	1.84	0.67
679	September, 1878, to October, 1879.	1.68	4.40	2.63	1.49	0.64
292	October, 1879, to December, 1880.	0.58	1.85	1.06	0.30	0.28
515	December, 1880, to April, 1881.	1.21	3.00	1.64	1.25	0.61
2527	Five epidemic periods.	6.37	17.35	9.25	6.16	2.57

* Brouardel, "On the Conditions of Propagation of Smallpox" (*Soc. Méd. des Hôpitaux*, December 9, 1870).

dinates of the hospital (servants or attendants of the sick) have been in the habit of clandestinely leaving the hospital to visit their friends outside, thereby violating the rules, which require of them a change of wearing apparel before going outside of the hospital precincts, and have thus been a cause of the propagation of the disease.

Hence, my conclusion is as decisive as that which I gave to the first question which came up in relation to these hospitals of isolation, and I would express it in the following terms: "Rigorousness of isolation in the special hospitals for infectious diseases is the surest means of preventing these hospitals from becoming centres of infection."

Having disposed of these two questions, we find ourselves confronted by a third question,—to wit, How are we to carry out this collective isolation? Must we build special pest-houses in connection with our general hospitals? Must we have separate hospitals for each infectious disease? Lastly, Where shall we locate these hospitals of isolation?

In the long and remarkable discussion undertaken by Fauvel and Vallin at the International Congress of Hygiene in 1878, we find the following conclusions, which give a definite answer to the various questions which I have just stated:*

1. The isolation which is the nearest perfection is that obtained by means of a hospital set apart for one single disease, or for several diseases treated under separate roofs. It is also the most expensive and the most difficult to organize.

2. A separate building in connection with a general hospital gives less, though still sufficient, security. Such lazar-houses are more convenient and easily managed than when far apart. The managing of these insulated hospitals, however, so that there shall be no exchange or communication with the other buildings in the midst of which they are situated, is a matter of difficult realization.

3. Isolation of patients suffering from contagious diseases in separate wards contiguous to the common wards is better than no isolation at all, but it is a method which is likely to disappoint, and end in failure.

We should endeavor as far as possible to have special hospitals for the principal infectious diseases, and this is the conclusion at which Chautemps arrives in his report presented to the Municipal Council on the hospitals of isolation.†

But this matter of the establishment of hospitals of isolation brings up at once questions very difficult of solution. The necessity of locating these hospitals in large fields away from human habitations has led our city authorities to seek suitable situations in the suburbs. But to this the suburban communes raise violent opposition.

In the important report of Colin made to the Council of Hygiene in 1888, our colleague has shown the necessity of erecting these hospitals outside of Paris, and especially such as are designed for the protection of the community against smallpox, measles, whooping-cough, and diphtheria. "I am of opinion," says he, "that the establishment of such pest-houses, which shall be constructed according to the modern notions of hospital architecture, will not have any injurious effects on the health of the suburban communes.‡

But when you have established in the outskirts of our large cities hospitals of isolation for the more infectious and contagious diseases, you have not done all. The patients must still be carried to these hospitals, and every means must be taken to prevent them, by the very fact of this transportation, from becoming active agents in the propagation of the infectious disease. To meet this want, we have instituted services of transportation, by which patients are taken to the hospital in special vehicles designed exclusively for this use.

These vehicles are disinfected after each trip, and, as before stated, they are used for no other purpose. At the present day, London and Brussels, as well as Paris, are provided with a service of this sort. Duchesne, in a recent memoir,§ well describes the mechanism of our Parisian service. A depot or central station situated at the Hospital St. Louis, another at the Hôtel Dieu, enable the sanitary authorities to have the carriages at a moment's notice at their disposal; and, in order that you may judge of the importance of these transportations, I will quote the statistics of such conveyances during the second half of the year 1887:

* Fauvel and Vallin, *Congrès International d'Hygiène*, 1878, p. 676.

† Chautemps, "De l'organisation sanitaire de Paris," 1888.

‡ Colin, "On the Construction in Suburban Places of Isolation Hospitals," August 20, 1888.

§ Duchesne, "On the Transportation to the Hospital of Patients affected with Contagious Diseases" (*Bull. et Memoires de la Soc. de Thérapeutique*, 15 Juillet, 1888, p. 536).

	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
Typhoid fever	20	33	45	25	64	63	250
Smallpox	96	120	80	75	97	108	586
Measles	10	5	4	1	5	9	34
Scarlet fever	47	25	12	24	13	20	108
Croup	10	11	9	22	15	22	89
Erysipelas	3	3	9	9	13	15	52
Other contagious diseases	6	7	11	4	4	9	41
	162	204	170	157	211	256	1160

Unfortunately, this transportation service, which has been in operation since 1881, is still but little known, and the eleven hundred and sixty patients above mentioned are far from representing all the cases of contagious affections which were taken to the hospitals during that length of time; and, if you refer to statistics, you will see that during the same period seventeen hundred and one patients affected with smallpox, measles, diphtheria, and scarlet fever found other means of transportation.

This is a deplorable fact, and numerous instances on record have shown what grave consequences the transportation of a patient affected with some contagious disease, as diphtheria, may have for other children who may happen at the same time to be riding in the same public vehicle, an omnibus, for example, or who may afterwards ride in the same carriage, the latter not having been disinfected.

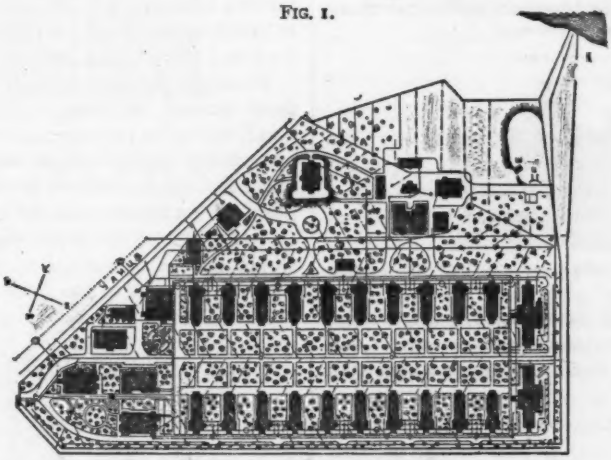
For less contagious affections, such as typhoid fever and tuberculosis, I do not believe that it is necessary to create special hospitals, but it is quite sufficient to group patients suffering from these diseases in wards by themselves, and this can be especially well effected in hospitals with insulated buildings.

The most complete arrangements of this sort that I have seen are in connection with the Alexander Hospital Barracks at St. Petersburg. In this hospital, a design of which I here show you (Fig. 1), each building, constructed on a uniform type, is destined for but twelve patients, and each is set apart for some special infectious disease.

These buildings, concerning which I have elsewhere written,* are elegantly built and furnished; their construction is peculiar, as you can see by the following cuts (Figs. 2, 3, 4, and 5)

In these hospitals with insulated buildings, this is the way the patients are received. If the disease with which the patient is affected is known at the time when he enters the hospital, he is transported immediately to the building set apart for that particular disease. When the diagnosis is doubtful, the patient is sent to the waiting ward (*salle d'attente*), where the disease is studied, and, when the diagnosis becomes positive, he is transferred to the pavilion destined for the malady from which he is suffering.

FIG. 1.



Plan of the Alexander Hospital.

* Dujardin-Beaumetz, in *Boston Medical and Surgical Journal*, January 3, 1889, page 23, "On the Russian Hospital Barracks, and, in Particular, the Alexander Hospital of St. Petersburg." See also the same Journal, February 21, 1889, page 201.

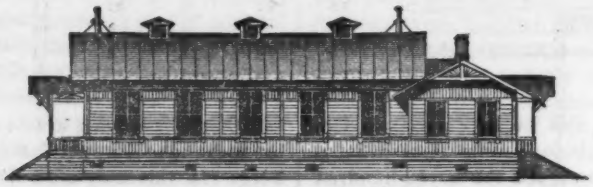
When a patient is admitted to the hospital, before entering the wards, and in the reception building, he is divested of his clothes, takes a bath, and puts on a suit of hospital clothing, while the garments that he previously wore are sent away to be disinfected.

The absence of corridors connecting these separate buildings, while rendering the medical service of the hospitals somewhat more laborious, is very favorable for the relative isolation of these infectious diseases. I believe, then, that this is the type of insulated pavilions that we ought henceforth to adopt in the construction of our future hospitals; at the same time I think that we might profitably increase the number of beds in each pavilion to twenty. Where there are only twelve beds in each building, as at the Alexander Hospital, a very large cortege of attendants and nurses is necessary. Thus, for the two hundred and fifty patients contained in this hospital, there is a retinue of one hundred and ninety-eight persons.

We might still further modify to advantage these isolated pavilions by adding to them a hall where the patients that are well enough and strong enough might take their meals, and which might serve at the same time for a place of recreation and for a reading-room. By this means the more vigorous patients would not be compelled to sojourn all the time in the wards of the sicker ones; and for these isolated pavilions I would propose the model which I here place before you, and which has been designed with great care by a pupil of the service, M. Burais. (See Figs. 6, 7, 8, and 9.)

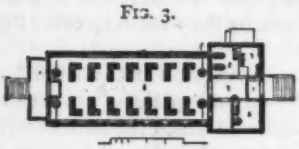
We might also take as models the double bar-

FIG. 2.



Barracks of the Alexander Hospital.

FIG. 3.

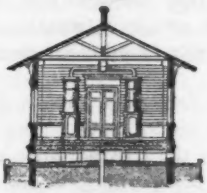


Plan.

FIG. 4.

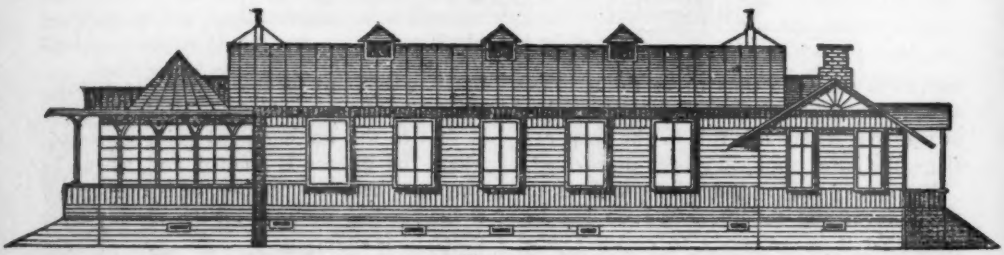
Façade.

FIG. 5.



Section.

FIG. 6.

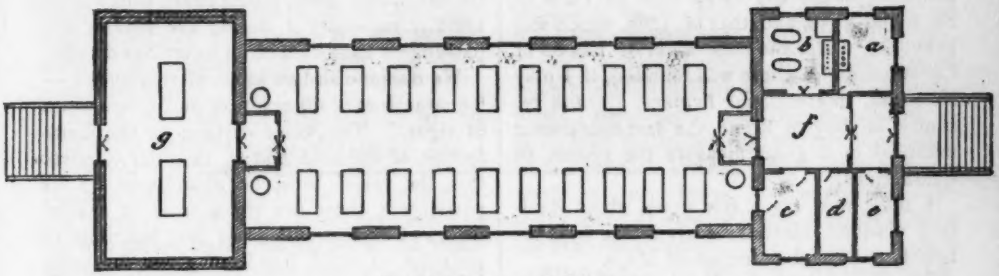


Elevation.

racks which serve for convalescent patients at the Alexander Hospital Barracks, the plan of which I have shown you (Fig. 10). A

ting of certain old-fashioned ships, which, like that of Tenes, are made over into receptacles for contagious diseases.

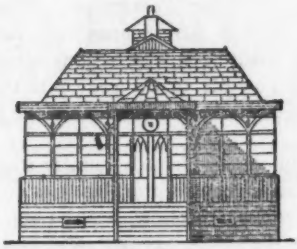
Fig. 7.



Project of a barrack building.

central building, devoted to the general service of the sick, and containing a large hall looking out upon a veranda which serves as

Fig. 8.

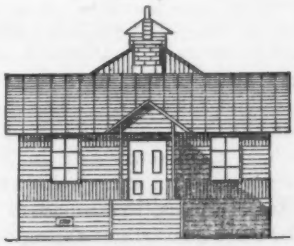


Façade.

a place of recreation and dining-hall for the patients, unites two similarly constructed barracks, in each of which there are fifteen beds.

Lastly, there is a mode of hospital isolation

Fig. 9.

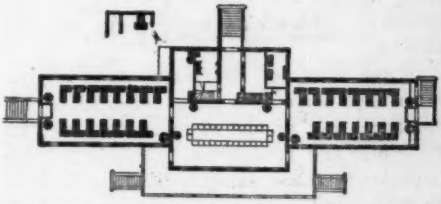


Section.

little employed at Paris, but of which our neighbors across the Channel make great use. I refer to the floating hospitals. You will find in the work of Lutaud and Hogg a description of two of these floating hospitals situated on the Thames,—the Castalia and the Atlas. To these hospitals on the Thames, we must add that of the port of Tenes. All these floating hospitals result from the refit-

Thus far I have been occupied with collective isolation in our hospitals, and with individual isolation. I must now briefly con-

Fig. 10.



Plan of barracks for the convalescent at the Alexander Hospital.

sider the grave question of quarantines and sanitary cordons.

The pestilential diseases, the plague, cholera, yellow fever, all originating in regions far away from France, can only affect the people of our country when they are imported here. Therefore, endeavors have been taken by sanitary boards to prevent the importation of these diseases.

Venice was the first—in 1348—to appoint a sanitary police. Being in continual commercial relations with the East, which was ravaged by incessant epidemics of the plague, Venice was compelled to institute means to oppose the introduction of these epidemics. She first appointed quarantine physicians, intrusted with the inspection of all sailing vessels entering her lagoons, then in 1403 she built a lazaretto, and from this time all cities having commercial dealings with her, made haste to construct similar pest-hospitals.

Many years afterwards, in 1821, the outbreak of the yellow fever in the south of France necessitated special measures, which were embodied in a law passed March 3, 1822. Lastly, the cholera made its appear-

ance in 1830, and from this moment it was mostly for defence against these two diseases, and especially cholera, that international sanitary conferences have been convoked. I must mention particularly those held in Paris in 1852 and 1859, and that of 1865, which was convoked under the inspiration of Fauvel at Constantinople, where was founded, if I may so speak, international hygiene. Quite recently, in 1885, at Rome, the last conference modified in a great measure the system of quarantines.

I must now say a few words about sanitary cordons, lazarets, quarantines, bills of health, and the disinfection of ships.

The name of *sanitary cordons* is given to a series of military outposts organized round a town or tract of country, so as to prevent egress of the inhabitants of a contaminated city, or ingress from a healthy territory. Such measures can only be resorted to in a country thinly settled, and especially where no very extensive commercial interchanges are carried on.

In the East and in Russia these regulations have sometimes been enforced to advantage. But as fast as railroads penetrate a country and commercial relations acquire importance, these sanitary cordons have to be abandoned, as relics of another age. Add, what still increases the causes of abandonment, that the troops themselves which compose this sanitary cordon may be attacked by the epidemic, and be in their turn the cause of the spread of the infectious disease which they are called upon to suppress.

If the sanitary cordons are intended for localities situated in the interior of territories, the lazarets and quarantines, on the other hand, concern our ports and our maritime cities.

From the fact that the diseases which we so much dread come from distant countries, which can have commercial dealings with our country only by the aid of navigation, it is mostly to our ports and sailing vessels that the laws and regulations of the sanitary police apply. The import of these laws is that no ship coming from a region where any pestilential disease prevails (cholera or yellow fever), can enter our ports without being delayed for a while in the waters outside the ports to give time for disinfection and for determining the sanitary condition of the passengers, in order that any infectious disease in the process of incubation might have the time to develop.

It is this time of delay, of variable length, to which has been given the name of quarantine, whatever may be its duration, though this may not be more than twenty-four hours; it enables the sanitary authorities to disinfect the cargo and crew, a procedure which is especially necessary if during the voyage any passengers have died of an infectious disease.

We distinguish two kinds of quarantine,—"quarantines of observation and quarantines of rigor." The latter pertains to the disinfection of the ship, and as, in order to effect this, the vessel must be abandoned by its passengers, these are transported to special places of retreat which are real hospitals of isolation, to which has been given the name of *lazaretto*. Generally these lazarettos are on an island, or they are constituted by pontoons, forming floating lazarets. Their organization leaves much to be desired, and this is true of all countries. These quarantine regulations are vexatious, and very damaging to maritime commerce. They profoundly disturb our commercial relations, and it is easily seen why nations are anxious as far as possible to do away with these restrictions on trade still in vogue at the present day.

No ship coming from a country where any pestilential disease is prevalent can enter our harbors without a bill of health; this bill of health is the ship's passport, and certifies to its sanitary condition from the time it leaves port to the time of entering port. We must distinguish bills of health from the official statement (*arraisonnement*), which is the captain's declaration respecting all the incidents of the voyage which in any way concern the public health.

In view of the numerous difficulties which beset this question of quarantines, attempts have been made to find ways to get rid of these vexatious measures. This subject was discussed with great thoroughness at the last International Conference held in Rome in 1885. Professor Proust, inspector-general of our sanitary services, has proposed to substitute for these quarantines the enforcement of rigorous measures of hygiene and disinfection during the voyage.

In the carrying out of this project, the rôle of ship physician would be a very important one, and a great responsibility would devolve upon him. He would have to decide at what moment and in what manner these measures of disinfection should be put in operation. Hence it is that government has proposed to appoint directly these physicians under its own responsibility.

Communicated to the Congress of Hygiene, held at Havre in 1887, this sanitary project, despite the support which Proust and Brouardel gave to it, has met with very violent opposition among the ship-owners and shipping companies, who profess to see in this direct appointment of a physician by the government an infringement upon their own rights and authority.

But it is to be believed that there will be ultimately agreement on this point, and I think that most hygienists will adopt the conclusions formulated by Proust in the following terms: "Most of the restrictions imposed upon commerce and navigation by the quarantines are but the consequence of want of observance on board the ship of the most elementary hygienic rules; these restrictions will disappear almost entirely the day that the ship managers shall be willing to adopt and carry out rational sanitary measures on shipboard."

As you see, gentlemen, this question of isolation brings up interesting points which I have only touched upon, for in order to do full justice to them, I should have to go over our whole hospital system, and enter into all the details of our sanitary regulations, as well as examine the complex basis of international hygiene.

We have seen in the foregoing lectures how an indivividual may be preserved from infectious diseases by disinfection and isolation. It remains now to examine another mode of preservation, which consists in creating by inoculations a medium refractory to infectious diseases, and in two following lectures on vaccine and attenuated virus I propose to study with you this other point of prophylactic hygiene.

ON THE SEPARATION OF MOTOR AND TROPHIC NERVES OF MUSCLE.

ALSO A CONTRIBUTION TO OUR KNOWLEDGE OF THE HEAT-REGULATING MECHANISM OF WARM-BLOODED ANIMALS.

By Geo. T. Kemp, Ph.D. (Johns Hopkins).*

Part II.

IN the first section of this paper the question of heat-regulation in warm-blooded animals was discussed, and the conclusion was finally reached, that the regulation of heat in such animals depended upon the relation between the production of heat in the tissues and the loss of heat from the body; and each of these factors was then shown to be under the control of the nervous system. Loss of heat is chiefly produced (in man) by vaso-dilatation in the skin, and heat production takes place mostly in the muscles. This production was furthermore shown to depend upon chemical activity of the muscular protoplasm (metabolism), and this metabolism to be directly influenced (not entirely controlled) by the nervous system. We recognized two kinds of nerve-fibres to the muscles,—the sensory and the motor; with a possible third variety,—the trophic.

After excluding the sensory nerves, it was left for this section to decide whether the motor nerves were really trophic in function, or whether an independent set of trophic fibres existed. Experiments have been made to show that cutting a nerve-trunk diminishes the chemical activity of the muscles which the cut nerve supplies. Such experiments clearly do not decide whether the *motor* nerves directly affect metabolism, for motor, some vaso-motor, and other fibres are cut together. Moreover, there seems to be such a clear relationship between the motor and trophic (?) fibres that an anatomical separation can scarcely hope to be effected. It, therefore, occurred to me to try to effect such a separation by minimal doses of curare, and see whether the production of heat went hand in hand with the function of motion. To do this, calorimetric measurements were made of the heat produced by animals under doses of curare just large enough to induce motor paralysis, or, at least, paresis, and then similar observations were made on the same and on different animals, to which much larger doses had been given.

A brief description of the calorimeter and its method of use is all that need be given here. It consists essentially of a metal box, containing water, placed inside a somewhat larger wooden box, and the space between the two packed with saw-dust. Into the metal box, which holds the water, a smaller metal box, with a tight, removable door, is made to go, so as to leave a space of a few inches between its sides and the sides of the water-box.

The animal is placed in the smaller box, which is then put into the larger one, with just enough water to cover it. The smaller box is provided with two pipes, soldered into its top, through which air is forced to supply the animal.

* Associate Director of the Departments of Physiology and Bacteriology, Hoagland Laboratory, Brooklyn, N. Y.

A large metal lid is then put over the two boxes, and the whole thickly covered with wool. The saw-dust prevents the loss of heat from the sides and bottom, and the wool from the top. A thermometer is let down into the water through a hole in the lid. By the proper methods it is then determined how many heat-units it takes to raise the water in the calorimeter one degree. This is the calorimetric equivalent, and, of course, varies with each calorimeter. When the dog is put into the box, and allowed to remain there for some time, the temperature of the water rises, being heated by the body of the dog. To find the amount of heat dissipated from the dog's body in a given time, we have only to find the number of degrees which the water has risen, and multiply this by the calorimetric equivalent. For example, if the calorimetric equivalent is 100, and the water has risen two degrees, the amount of heat dissipated is clearly 200, for the calorimetric equivalent is the number of heat-units required to raise the water one degree. To find the amount of heat produced, the temperature of the dog is taken before entering and after leaving the calorimeter. If the dog's temperature has risen, then the dog must have produced not only heat enough to raise the water of the calorimeter so many degrees, but also the heat required to raise his own temperature. This latter amount is found by multiplying the specific heat of the dog by his rise in temperature, and this, when added to the heat recorded by the calorimeter, gives us the total heat produced. If his temperature falls, the heat-units represented by the fall are calculated in a similar manner, and the result is subtracted from the amount of heat given to the calorimeter; for the amount subtracted clearly came from a cooling of the dog's body, and represents heat stored up there before he went into the box. The work was done at the Physiological Laboratory of the University of Pennsylvania, and the calorimeter employed was the one used by Professor H. C. Wood in his well-known research on fever.*

For a full description of this apparatus the reader is referred to that memoir. Fahrenheit thermometers were used, and English heat-units adopted, for the reason that the calorimetric equivalent had already been calculated in these terms and not in calories, for readings of the Centigrade thermometer. The thermometers were graduated to $\frac{1}{10}$° F., and the readings could easily be estimated to $\frac{1}{20}$°. The thermometers were carefully compared with a standard just before beginning the work, and the corrected readings are given in every instance.

In order to avoid the thermic disturbances incident to the use of chloroform or ether, the dogs were tracheotomized under an anæsthetic the day before the curare was given. The muscles were separated so as to afford ready access to the carotid artery, care being taken not to disturb its sheath, thus avoiding thrombosis in the artery or irritation of the vagus. The whole operation was performed antiseptically and the wound dressed with iodoform. The next day the wounds were found devoid of inflammation and the dogs in good condition. After giving curare, the stitches were cut, and, without further operating, the carotid could be found, and separated from the vagus for connection with the kymograph. Ordinary surgical tracheotomy canulas of large size were used, so that the inside tube could readily be removed and cleansed of mucus. The artificial respiration was kept up sometimes by a bellows driven by an engine, and at others by a large rubber gas-bag under pressure. This bag was connected by a T-tube with a Bunsen pump, which constantly forced air into it, and also with the dog, a cut-off being placed in this path, which allowed the air to pass at regular intervals to the dog. The engine and bellows were always used when the dog was in the calorimeter. The air to the dog was forced through a Woulff's bottle containing some water, in order to avoid the great thirst and consequent disturbances from pain which always follow long-continued artificial respiration with dry air.† The simple tying down of a dog is nearly always followed by a considerable fall in temperature. An experiment with curare was never begun until such fluctuations of temperature had disappeared, although a delay of several hours was often necessitated. An ordinary calorimetric experiment was then conducted as follows: The dog's rectal temperature was taken while he was held by an assistant, without being

* "Fever: A Study in Morbid and Normal Physiology" (*Smithsonian Contributions to Knowledge*, No. 357).

† The same bottle and water were always used when the dog was put into the calorimeter without curare. The same difference between the temperature of the water in the calorimeter and the temperature of the air in the room was carefully maintained, so that the error, from condensation of moisture in the calorimeter, was the same in each case.

tied down. He was then put into the calorimeter, without giving him curare, and the artificial-respiration apparatus, which was later to supply him directly with air, was made to keep up the ventilation of the calorimeter-box. At the end of an hour he was taken out, and his temperature taken without tying him down. He was then allowed to run around the room for an hour or more, after which he was tied down, and remained so until his temperature neither rose nor fell, as shown by a thermometer remaining in his rectum.

With a sharp knife the jugular vein was then laid bare and a canula inserted. This can often be done without even a kick on the part of the dog. Curare was next injected, and the temperature record taken every minute or two until respiration stopped (from about two to ten minutes).

The artificial-respiration apparatus was then connected with the dog, and his temperature taken every few minutes until the proper time to put him into the box, which varied with the observation to be made.

In the first section of this paper* it was shown that the vaso-motor system had much to do with the distribution of heat through the body, and in man was one of the chief means by which the body rid itself of an excess of heat. It was, therefore, very necessary to understand clearly the effect of curare on the heart and vaso-motor system, in order to eliminate any errors which might be introduced through these media. Numerous observations were made with doses of curare, varying from those which were just sufficient to paralyze the motor nerves of striped muscles, to large doses, which nearly reduced the blood-pressure to *nil*. If it were practicable to publish all the curves obtained in this study they would speak for themselves, but as this, of course, is out of the question, I shall simply state as briefly as possible the results of my observations, referring to only two of the curves which were taken.† Small doses of curare may or may not lower the general arterial tension, the resistance of the dog to curare varying greatly with the individual. Small doses often show a weakening of the respiratory undulations on the blood-pressure curve, due to a paresis of the respiratory muscles. A slightly increased dose will cause the respirations to become very feeble or to stop altogether, and the pressure-curve rises, owing to asphyxia. That the rise is really due to asphyxia is shown by the fall of the curve the moment that artificial respiration is started. All this while the heart and vaso-motors are often totally unaffected. A larger dose will cause the curve to fall, the action being principally on the heart. The heart-beats are greatly diminished in power, this effect being often seen before the respirations are entirely stopped by the drug. The vaso-motors are still unaffected to any appreciable extent, for stopping the artificial respiration causes an immediate and decided rise in the curve, followed by the development of Traube curves.‡

After larger doses of curare, stopping artificial respiration will cause the pressure to rise, and Traube curves to appear; but the rise is less marked, the heart-beats very much more feeble, and the fall from exhaustion comes much sooner than with smaller doses. The rise is undoubtedly due to vaso-constriction, for the heart-beats are not increased sufficiently to account for it, and furthermore the Traube curves show that the vaso-motors are still functional.

The effect of curare on the *bodily temperature* has been studied by various observers with different results. Tscheschichin§ found that the temperature fell from the time of injection until the first convulsion, and then rose. He attributed the rise to the muscles, not by virtue of the twitches and jerks which they exhibit, but by an increased heat-production in them.‖ Falk¶ found that the tem-

* Page 86.

† See curve 3, at end of article, and explanation of curves on pages 162 et seq.

‡ Plate III. Traube curves are the prominent long undulations in the blood-pressure curve which are seen in curarized animals. They cannot be due to changes in thoracic pressure, as are the ordinary respiratory waves, for the muscles of the thorax and of the diaphragm are completely paralyzed. They are explained as follows: Curare, in moderate doses, acts only on the nerves to striped muscles. The respiratory centre in the medulla, not being affected by the drug, continues to discharge, but its nervous impulses, on their way to the muscles of the thorax and diaphragm, are blocked by the palsied nerves. The vaso-motor centre in the medulla discharges sympathetically with the respiratory centre, and, as the curare does not paralyze the vaso-motor nerves (they being to unstriped muscle) as readily as those to the voluntary muscles of respiration, the vaso-motor impulses generated in the medullary reach their peripheral termination in the arterioles and cause a constriction.

§ *Arch. f. Anat. u. Physiol.*, 1866, p. 160.

‖ "Leichte Zuckungen haben übrigens gar keinen Einfluss auf die Temperaturerhöhung, . . . so muss man die Temperaturerhöhung während der Krämpfe als Folge der gesteigerten Wärmeproduction in den im erhöhten Thätigkeitszustande befindlichen Muskeln."

¶ *Pflüger's Arch.*, vol. xxv. p. 590.

perature sank for the first five minutes, then rose to the normal (or generally above), and then fell rapidly, shortly before death. Fleischer* gets no fall before the rise, and thinks the convulsions have nothing to do with the rise. Riegel† attributes the fall which follows the administration of curare to a diminished metabolism.

Voisin and Liouville,‡ as early as 1866, relate clinical observations with curare in which the drug was pushed, even to partial paralysis, with most interesting results. They found that after hypodermic injections of curare there resulted fever with chill, followed by sweating, with profound nervous disturbances, such as general muscular twitching, difficulty in swallowing, facial contortions, incoördination of movements of the eyes and of the limbs, and even paralysis of voluntary motion in the limbs. The temperature in some cases rose to 40.4° C. (= 102.7° F.). Voisin and Liouville think that the phenomena here observed (including the rise of temperature) are to be attributed to increased sensibility of the nerves, thus agreeing with Claude Bernard, whom they quote as follows:§ "Lorsque les propriétés vitales d'un nerf sensatif ou moteur vont disparaître sous l'influence d'une perturbation quelconque elles s'exaltent d'abord pendant un certain temps, il se produit une surexcitation plus ou moins forte qui chez les lapins soumis à l'influence du curare se traduit par des legers frissonnements dans les muscles peauciers."

In my experiments I find that small doses of curare cause an immediate rise in the rectal temperature, the extent and duration of which depend upon the dose and the temperature of the surroundings. Large doses cause a slight transient rise, followed by a steady fall, the rapidity of which depends somewhat upon the dose, but especially upon the temperature of the surroundings.

The rise in temperature I would explain by an increased sensibility of the nerves, stimulating metabolism in the muscles. The fall I believe to be due chiefly to diminished heat-production, as I hope to show after a discussion of the results of my calorimetric observations. A detailed account of the complete series of my experiments would involve all the figures incident to the calculation of thirty-seven calorimetric observations, besides long lists of temperature-readings which it would be impossible to give in anything short of an extensive monograph. I have, therefore, condensed the work into a table, in which the results of the experiments are given *seriatim*. Whenever there is anything in connection with an experiment which would affect the result, it will be found in the column of remarks.

EXPLANATION OF TABLE.‖

Columns B and Q give the dose of curare administered, B giving the total amount of the dose, and Q the amount of curare per pound of dog. The figures in Q are directly comparable.

C and D give an idea of the rapidity with which the curare took effect.

E and F show the changes in rectal temperature of the dog under different doses of curare.

I and J show the rectal temperature on entering and on leaving the calorimeter respectively.

K is the difference between I and J, and offers a ready means of comparing the relative power of dogs to maintain their temperature before receiving curare, and after different doses of that drug.

L is the total heat dissipated, given in English heat-units, for one hour. M is the hourly heat-production in the same terms. N is the difference between L and M. O shows the effect of curare on heat-dissipation, and P on heat-production. R shows the amount of heat produced per pound of dog. The figures in this column are directly comparable, and are arranged conveniently for showing the effect of curare in large and small doses upon the heat-production of the dog as compared with the normal.

In glancing over column K it will be seen that there is a tendency for the temperature of the dog to fall while in the calorimeter, even when no curare had been given. The variations under curare are much more frequent and much greater than in the normal dog, showing that curare causes a disturbance of the heat-regulating system more or less profound. In looking at column K more carefully, it will be seen that in every case where the temperature of the dog rises, the dose of curare is small (at least less than one milligramme ($\frac{1}{64}$ gr.) per pound, as seen in column Q), while a larger dose of curare (2.84 and over, in column Q) invariably causes a con-

* *Pflüger's Arch.*, vol. ii., 1869, pp. 433–444.
† *Centralbl. f. d. Med. Wiss.*, 1871, p. 403.
‡ *Gaz. des Hôpitaux*, 1866.
§ Op. cit., p. 443.

‖ See end of this article.

siderable fall. It has already been shown that curare in large doses lowers the blood-pressure, and it might be urged that the loss of temperature was due to an increased heat-dissipation, such as follows section of the cord. That this is not the case becomes evident from a comparison of columns P and O. In nearly every case in which the number in P has the negative sign, and its value is considerable, the corresponding number in O will be found to have the negative sign also, but to be less in value than F. This shows that heat-production (column P) is more influenced by curare than heat-dissipation (column O), the heat-dissipation being even less than in the normal dog. This fact is suggestive, for we have seen that curare causes a fall of blood-pressure, but does not paralyze the vaso-motor nerves,* and it would seem that when the heat-production is greatly lowered, the vaso-motor nerves so control the supply of blood to the skin as to allow as little heat-dissipation as possible. The effect of decidedly large doses on heat-*production* is readily seen by comparing the columns Q and R, for example. Where the dose in Q is 3.75, we have the hourly heat-production reduced from 4.8 to .7 per pound of dog, or from 76.04 to 11.08 for the whole dog, and it will be seen that, without exception, a large dose of curare in Q always has a corresponding number in R, which is less than the number for the dose .00 of curare. The same thing is shown in column R, in those experiments in which three observations were made in one day. The effect of the first dose may be either to increase or diminish the heat produced, but after the second dose it is invariably smaller than the normal, the difference being generally very striking.

Experiment 0.—This was one of the first made, and the dose of curare was not recorded. The curare was injected subcutaneously and in considerable quantity, for the minimal dose of curare had not been found when this experiment was made. The heat-regulating mechanism was disturbed to an enormous degree, for there was an increase in the amount of heat dissipated, and not only was there no compensatory rise in the amount of heat produced, but, on the contrary, the nerves regulating heat-production were so completely paralyzed that the heat produced fell 51.36 below the normal. On the other hand, heat was given off freely from the dog despite the diminished production, and both heat-production and heat-dissipation worked together to lower the rectal temperature.

March 26.—The dog received 60 milligrammes of curare (3.75 milligrammes per pound) subcutaneously. The heat-production fell from 76.04 to 11.08. The heat-dissipation was only diminished from 100.40 to 90.28.

March 31.—Large dose of curare (4.55 milligrammes per pound) subcutaneously. Heat-production fell 29.63 ; heat-dissipation, 16.33.

April 2.—Large dose of curare to same dog, which had recovered from last experiment. The dose given was the same as in March 31. Heat-production fell 57.08 ; heat-dissipation, 23.18.

April 15.—Moderate dose (1.42 milligrammes per pound) injected into the jugular. The effect was very marked. Heat-production fell 99.78 ; heat-dissipation, 45.14.

May 5.—Small dose (.85 milligramme per pound). The heat-production and heat-dissipation are each increased. The heat-dissipation is increased slightly more than the heat-production. This may be explained as an effort of the vaso-motor system to get rid of an excess of heat produced by the stimulating effect of a *small* dose of curare, or it may be regarded as an effort of the metabolic nerves to produce more heat to compensate for the increased dissipation. The difference is so small that the experiment may well be regarded as one in which the animal is practically normal except for the loss of power of motion.

May 9.—Small dose (.67 milligramme per pound), causing a decided rise in the amount of heat produced. Here the rise cannot have been produced by thermotaxic influences tending to compensate for increased heat-dissipation, for the dissipation was slightly less than in the normal dog. We are forced, therefore, to regard the increased heat-production as a direct result of a stimulation of the metabolism by the small dose of curare, and this, indeed, by its action on the nerves.† The fact that the power of motion returned to the dog while in the calorimeter may partially account for the largeness of the increase in heat-production in this experiment, but this is in common with several other experiments, and will be discussed separately.

* Page 157.

† Compare Colosanti, *Pflüger's Arch.*, 1878, vol. xvi. pp. 157–172. Referred to in the first division of thi paper, p. 88.

May 10.—In this experiment two doses were given to the same dog, first a small one (.68 milligramme per pound), and afterwards a large one (2.84 milligramme). The same remarks apply here, for the small dose, that were made for the experiment of May 9. The heat-dissipation remained absolutely the same as in the normal dog, while the heat-production was increased 20.66. After the larger dose the characteristic phenomena of poisoning from full doses of curare were exhibited. The heat-production was diminished 82.32, while the heat-dissipation was only diminished 19.52. The numbers in column R are very striking in this experiment. They are 6.2 normal, 7.3 small dose, 1.8 large dose.

May 12.—(Small dose, 1.00 milligramme per pound.) Heat-production and heat-dissipation both increased almost exactly to the same extent.

May 18.—Small dose (.86 milligramme per pound), followed by a decided fall in heat-production. Here we have an apparent exception to the rule observed in the previous experiments, but a closer examination of the experiment shows that it is what we should expect. We suppose small doses of curare to increase the metabolism in muscle, through the nerves by rendering them hypersensitive, which by no means destroys their thermo-*regulating* function, however that may be performed. Now, in the present case, for some reason not apparent, the heat-dissipation fell from 254.98 to 198.60; and, if the same amount of heat had been produced as in the normal dog, we should have had the temperature raised to 105.4°, which would have shown an embarrassment of the thermotaxic system not generally produced by small doses of curare. This experiment, then, is quite in accord with the others, and shows that the nerves had not lost control of heat-regulation.

May 19.—Moderate dose (1.43 milligrammes per pound). Heat-production diminished only 4.14. The disturbance of the heat-regulators was greater than this, however, as there was an increase in the heat-dissipation, and had the thermogenic nerves been fully functional there should have been an increase in heat-production to compensate for this extra loss.

June 9.—Small dose (.40 milligramme per pound). The record of this experiment is rather peculiar. The dog came under the influence completely in eleven minutes, and his temperature rose with astonishing rapidity until it reached 105°, a gain of 2.4° F. in one hour and twenty-five minutes. The dog was then put into the calorimeter, in which his rectal temperature fell 3.4° in one hour, and the experiment gave results precisely like those in which large doses of curare had been given.

June 10.—In this experiment two doses were given to the same dog. The first one small (.46 milligramme per pound), the second larger (1.09 milligrammes), making in all 1.54 milligrammes. The small dose caused an increase in heat-production (5.8 per pound over 5.6 in the normal dog), but not sufficient to meet the increase in heat-dissipation, showing that the heat-regulating mechanism was embarrassed. The larger dose caused a diminution in heat-production of 78.86, and in heat-dissipation of 66.96. The figures in column R of this experiment are instructive.

June 21.—In this experiment, again, the dog received two doses,—first, a small dose (.27 milligramme per pound), and then a larger one (.92 milligramme per pound), making 1.19 milligrammes per pound in all. Here, as in the experiment of June 9, we have a prompt absorption of the curare, a rapid rise of temperature to 104.8° (a rise of 1.35° F. in thirty-four minutes), and a subsequent fall in heat-production, with a considerably smaller fall in dissipation, just such as usually results from larger doses of curare. The second dose of curare was not large, and, strange to say, appeared to act as a stimulant to heat-production,—the heat-production per pound falling from 6.1 to 4.0 under the small dose, and then rising to 4.8 under the second small dose.

June 23.—Dog received two doses,—the first, .24 milligramme per pound; the second, 3.00 milligrammes per pound. The small dose, as in the two previous experiments, gave results identical with those produced by larger doses earlier in the series of experiments, although not so marked. The heat-production fell 26.68, while the fall of the heat-dissipation was 20.74. The larger dose produced the characteristic effect, the heat-production falling 69.25, and the heat-dissipation 56.12. It will be observed in all the experiments made in June that a smaller dose of curare had far more paralyzing action than earlier in the spring. It is interesting at least to note the observation, although no explanation is apparent.

As far as we have now proceeded in a discussion of these experiments, we have corroborated the testimony of the observers quoted in the first section of this paper, in

finding that curare lowers metabolism. We have accomplished this by methods not hitherto employed for research in this particular field. We agree with Tscheschichin that the muscles are the seat of increased heat-production in effecting the rise of temperature caused by curare, and agree with Bernard and Voisin and Liouville that this is probably due to increased sensibility of the nerves, but the question still remains unanswered,—Are the nerves in question the ordinary motor nerves to muscle, or is there a special set of trophic fibres? In glancing over column R it will be seen that the only cases in which the heat-production is increased above the normal are those in which the dose of curare is small, and in the column of Remarks it will be found that in every such case there were evidences of motor power, such as twitchings of the muscles, or, in some cases, even a regaining of the power of respiration and voluntary movement. The reason for this is that, in seeking a dose with which to paralyze the motor nerves and leave all others intact, we have to use such small quantities of curare that the paralysis is not perfectly maintained for an hour. Of course, a hard-and-fast proof that there are two sets of nerves cannot be given until one can be completely thrown out and the other left perfectly functional, but in the present instance, I hope to show that there are sufficient grounds for believing that in a very high degree of probability there is a set of trophic nerves separate from the motor, although the two are probably closely related. That the increased heat-production which follows the administration of small doses of curare is not due to heat generated in the muscles by their twitching is shown by such experiments as that of March 31. Here the dog was given a large dose of curare (4.55 milligrammes), and was observed continuously for seven hours thereafter, when the first signs of returning motion made themselves manifest by the characteristic twitchings. The dog was then put into the calorimeter, and when taken out twitches and strong jerks occurred, but no co-ordinated movements. The results of the experiment are those of a typical calorimetric experiment after large doses of curare, and do not resemble at all the results of minimal doses. In the experiment of May 5 the dog was given .85 milligramme of curare per pound, and, after waiting nearly two hours, was put into the calorimeter just when the twitchings were beginning to appear. In both these cases the twitchings were going on all the time that the dog was in the calorimeter, and yet they made no appreciable impression on the result. In all the experiments except the two just mentioned the dog was completely paralyzed on entering the box, and from observations on animals under curare out of the calorimeter, we know that the dog must have been completely paralyzed the greater part of the time he was in the calorimeter. It, furthermore, seems by no means probable that a dog with such a strong paresis that he could only give an occasional twitch or jerk, should be able to call his palsied nerves into such activity as to give greater metabolism under those circumstances than in the normal condition; and, on the other hand, on comparing the results in the different experiments in which the heat-production was increased under small doses of curare, we find, even when the dog had sufficiently recovered in the calorimeter to breathe and walk, there was little or no difference in the increase of heat-production over cases where simple twitchings were seen. Compare May 12 with May 9 and 10. Another observation to the point is, that in all the June experiments, in which small doses of curare were given (with one exception), the characteristic twitchings were seen, and yet in some cases there was a fall in heat-production greater than in others where the motor paralysis had been complete from beginning to end. Surely this would not have been the case if it could be argued from the presence of twitches and jerks that the motor nerves retained their control over heat-production, and, if the twitchings are due to an increased sensibility of the motor fibres, it is still more absurd to suppose that they would be more active in the performance of one function (motion), and less active in the performance of another (heat-production). We may then regard it as demonstrated that minimal doses of curare produce a strong paresis of the motor nerves without thereby lowering heat-production; hence it may be inferred that *the motor nerves are not the nerves which control the metabolism in muscle by which heat-regulation is maintained.*

In most cases doses of curare which are just sufficient to paralyze the motor nerves cause a rise in bodily temperature, which the calorimeter shows to be due to an actual increase in heat produced. Larger doses invariably cause an enormous fall in heat-production. Now this is directly in accord with the law of Claude Bernard, previously

stated,* that drugs which paralyze any set of nerves first produce a condition of increased sensibility of these nerves; therefore we may say that curare paralyzes both motor and trophic nerves, the motor nerves being, as a rule, more susceptible to its action. Very small doses of curare produce an increased sensibility of both motor and trophic nerves. This is well shown in the experiments of Voisin and Liouville already given. A slightly larger dose is sufficient to paralyze the motor nerves, leaving the trophic nerves still in their hypersensitive condition. A large dose paralyzes both motor and trophic nerves. Besides the evidence here adduced there are certain pathological conditions which strongly suggest that the motor and trophic nerves are separate, though probably closely related. In the peculiar disorder known as Charcot's disease, swelling and erosion of the joints occur, together with degenerative changes in the muscles, following lesions of the anterior cornua of the spinal cord. In progressive locomotor ataxia these arthritic lesions have also been found associated with degeneration in muscle.

After a careful examination of the cord in these cases, Charcot† found that the anterior cornu of the cord was involved, and always the anterior cornu on the side of the arthritic lesions. In some cases the trouble in the joints was well advanced before the motor power of the muscles was lost, so that the objection of disuse may be laid aside.‡ As the motor nerves to the joints are out of the question, we are forced to conclude that there are separate trophic fibres which have their origin in the anterior cornu along with the motor nerves, and which are, therefore, probably in close relationship with them.

SUMMARY.

The results of the experiments on the influence of curare on the vaso-motor system, with references to curves, will be found on pages 163 to 165.

Curare produces more or less profound disturbance of bodily temperature in warm-blooded animals.

The injection of small doses is followed by a rise in bodily temperature.

After the injection of large doses the bodily temperature falls. This fall is preceded, however, by a rise more or less transient.

The rise from the small dose is brought about by an actual increase in the heat-production of the body.

The fall, while sometimes aided by vaso-motor phenomena which cause increased heat-dissipation, is regularly due to a lessening of the amount of heat produced.

The motor nerves are not the nerves by which the thermotaxic nervous mechanism effects a greater metabolism in the muscles, with the consequent increase in the production of heat.

Such heat-regulation is wrought through nerves distinct from the motor, though probably closely related to them.

EXPLANATION OF PLATE III.

Upper Curve.—Dog had been put under ether for tracheotomy, and exposure of the jugular vein for injection of curare, and had then been allowed to come from under the ether while still tied down. 8.5 milligrammes of curare were next given, and it required eighteen minutes for all reflexes to disappear. This is a very small dose, but was sufficient to produce complete muscular paralysis. Upon shutting off artificial respiration, marked by x on the curve, both pressure and heart-beat were affected in a most beautifully rhythmical manner. A large dose of curare given just after, and not shown in curve, produced the typical fall in pressure and weakening of the heart-beat. The peculiar character of the curve just after the stoppage of artificial respiration strongly suggests a hypersensitive condition of both vaso-motor and cardio-inhibitory nerves or centres. We have the strong heart-beat on the descending limb, and the quickening of the heart-beat on the ascending limb; in short, we have a greatly exaggerated condition of what occurs normally, though not a twitch was observed in either thoracic muscles or diaphragm. It is interesting to note that several curves almost exactly like the one given, were obtained from the same dog before muscular movement ceased, but while the respiration was embarrassed.

Lower Curve.—This is a continuation of the upper curve, the distance from base line being true for each, and was taken somewhat later. The dog, in the meanwhile, had received two hundred and four milligrammes of curare. Upon shutting off arti-

* Page 158.

† "Leçons sur les Maladies du Système Nerveux." Paris, 1884, p. 121.

‡ Charcot's discussion of the whole subject is most masterly, and references to literature are given, so that for a fuller discussion of the subject the reader is referred to that author. Op. cit.

ficial respiration the pressure rose and Traube curves appeared, but they were nothing like as large as in the preceding curve. A continuation of the curve showed also that vaso-motor exhaustion soon came on, and the pressure fell. The heart-beats, though stronger, are not so much affected as in the upper curve, thus showing that the cardio-inhibitory nerves are affected as well as the vaso motors, when large doses of curare are given.

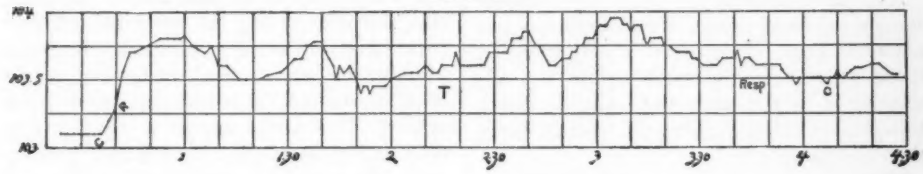
EXPLANATION OF PLATE I.

Ordinates, temperature; each four lines = 1° F.

Abscissas, time; each line = ten minutes.

This curve was taken from the dog in the experiment of June 23.

PLATE I.



12.20. Temperature of room, 81°.

12.35 C. Curare injected, eight milligrammes,—.52 milligramme per pound.

12.39 R. Orbital reflex just disappeared.

2.15 T. Twitchings in abdominal muscles, being first signs of returning motion.

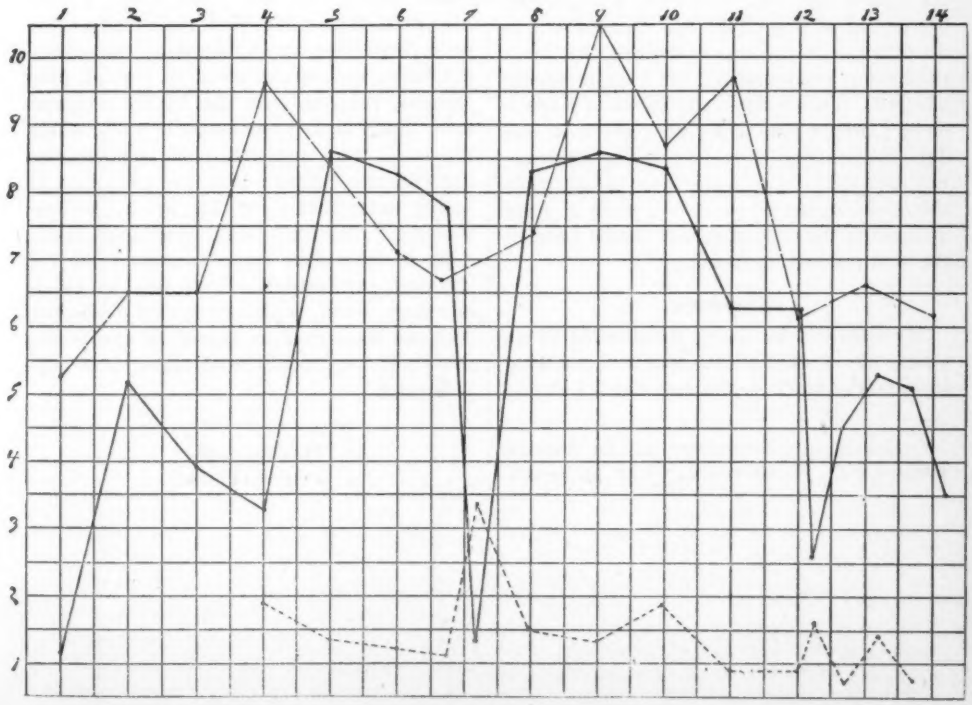
3.43. Slight signs of natural respiration.

3.55. Respiration strong.

4.06 c. Curare again injected, .24 milligramme per pound.

It will be seen that there is a decided rise in the curve after the injection of curare. The curve shows admirably what a peculiar influence small doses of curare have on bodily temperature. This rise of temperature from curare is what is always observed, and takes place even when there is a subsequent fall. It was not deemed necessary to give other curves, as this one is essentially a type of those obtained in all the experiments.

PLATE II.



EXPLANATION OF PLATE II.

This plate is intended to give a bird's-eye view of those experiments in which the effects of large and small doses of curare are compared.

The values are all in terms of ordinates, the abscissas representing the different experiments.

The upper curve ———— represents the number of heat-units produced per hour, per pound of dog, when no curare was given.

The curve marked ---------- represents the dose of curare in milligrammes per pound of dog. This curve is only given in those experiments in which curare was administered in the same way,—*i.e.*, by direct injection into the jugular, the results being thus comparable.

The curve ———— represents the number of heat-units produced per hour, per pound of dog, after giving the dose of curare indicated by the dotted curve.

When the curve ———— rises above the one represented by the line ————, it shows that the actual heat-production, after giving curare, was greater than in the normal dog. In every such case the dose of curare was between .4 and 1 milligramme per pound of dog, which was about the dose found to produce paralysis or strong paresis, and at the same time stimulate metabolism. When the dose is increased above one milligramme per pound of dog, there is a marked fall in the curve ————, showing that heat-production (metabolism) has been markedly affected. This is especially shown in Experiment 7, where two doses were given in one day, the smaller dose—6.2 milligrammes—kept the curve ———— considerably above the curve ————, while a great increase in the dose of curare almost lowered the heat production to *nil*.

ADDENDUM.

My attention has lately been called to an article by Mosso,* which appeared not long since in *Virchow's Archiv*, and which I should have been glad to mention had I noticed it sooner. My paper was in the printer's hands when I read Mosso's article, but of such interest is it in connection with the subject in hand that I add this brief notice of it as an addendum.

In working with curare on both frogs and dogs, Mosso obtained the characteristic rise in temperature which I have figured in Plate I, and adds another observation of exceeding interest, that, after giving strychnine, the rise is more prominent than before. He finds, also, that when a dog has received sufficient curare to paralyze his motor nerves, and even when his temperature has been lowered, a dose of strychnine will cause his temperature to rise again.† This would appear to conflict with the conclusions which I have drawn from my work, but he adds‡ that when the dose of curare is large this rise will not take place, which shows that his observations and mine are confirmatory of each other, for when the rise did take place he evidently had struck the proper dose of curare to paralyze the motor but not the trophic nerves.

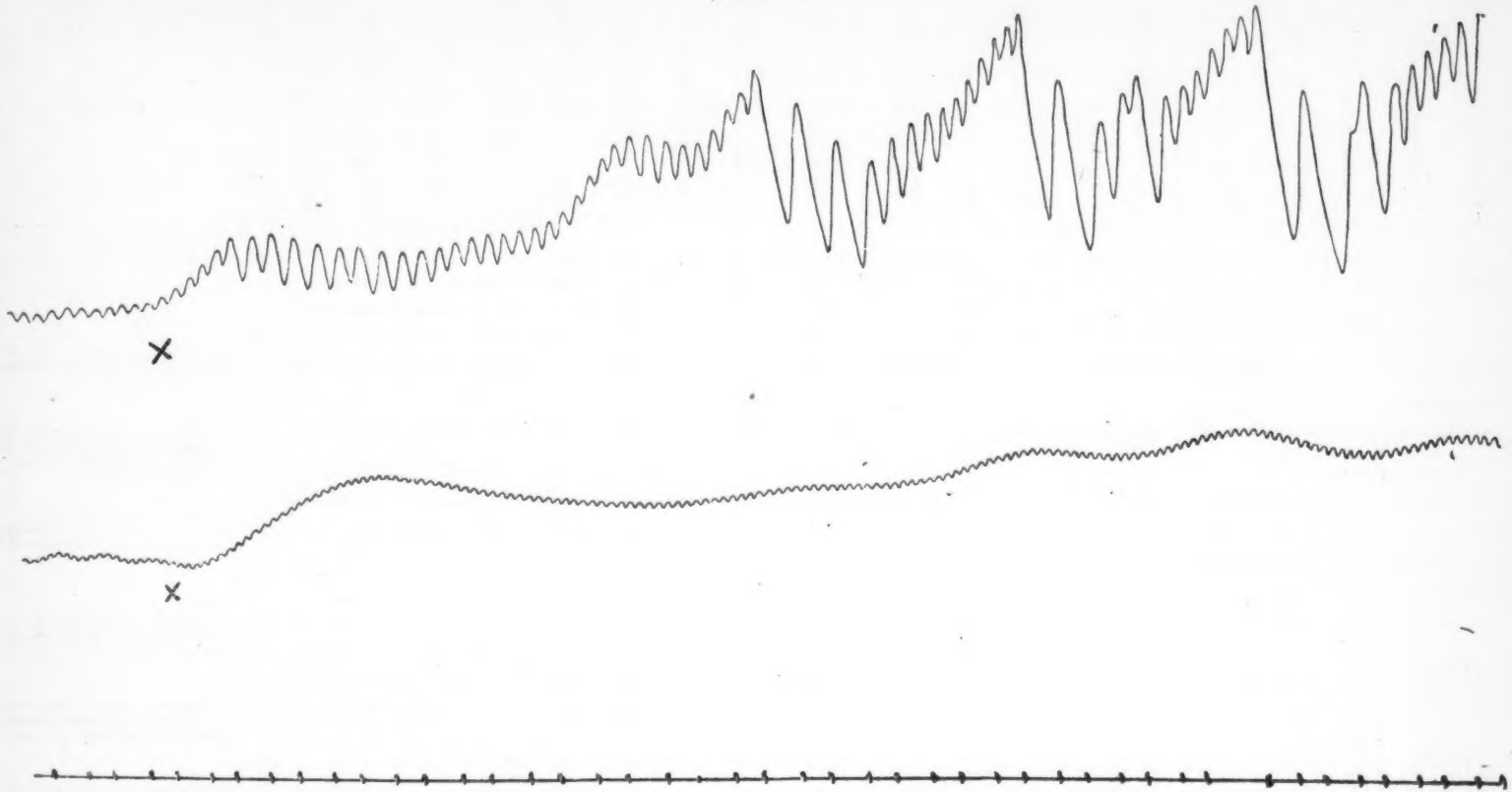
In addition to this, he has shown that the temperature-curve of an animal does not run parallel to the curve of work done by its muscles; and the drift of his whole work is towards the same conclusions that I have given at the end of my paper,—viz., that the motor nerves are not those which govern heat-production. His work embraced a more widely-extended study than mine into different fields, such as the effect of psychic phenomena, pain, etc., on the bodily temperature; but it lacked one essential point, which my work has supplied,—viz., calorimetric observations. Mosso took the variations in bodily temperature as his guide, without showing conclusively what produced them. I have *shown* that these variations are, for the most part, actually due to what he *attributes* them,—viz., variations in heat-*production*. The two researches are, therefore, concerning this point, reciprocally confirmatory and supplemental.

* "Einfluss des Nervensystems auf die thierische Temperature." *Virchow's Arch*, vol. cvi. pp. 80–126.

† Op. cit., p. 98.

‡ Op. cit., p. 114.

PLATE III.



Reference number.	Date of experiment.	A. Weight of dog (pounds).	B. Dose of curare in milligrammes.	C. Time after injection of curare that respiration stopped.	D. Time after injection of curare that orbital reflex disappeared.	E. Temperature of dog before injecting curare.	F. Gain (+) or loss (—) of dog's temperature between injecting curare and entering calorimeter.	G. Temperature of room.	H. Time after injecting curare that dog entered calorimeter.	I. Temperature of dog entering calorimeter.	J. Temperature of dog leaving calorimeter.	K. Gain (+) or loss (—) of dog's rectal temperature in calorimeter.	L. Total heat-dissipation per hour.	M. Total heat-production per hour.	N. Excess (+) or deficiency (—) of heat produced as compared with heat dissipated.	O. Difference in heat dissipated before and after curare.	P. Difference in heat produced before and after curare.	Q. Dose of curare in milligrammes per pound of dog.	R. Heat produced per pound of dog.	Remarks.
0	.....	22	00	.....	.....	.....	.....	63°	.....	104 60°	104.50°	— .10°	176.90	175.25	— 1.65	.....	.....	00	7.8	
.....	.....	22	.....	.....	.....	.....	.....	66°	25 minutes.	99.70°	95.20°	— 4.50°	197 64	123.39	— 74.25	+ 20.74	— 51.36	.....	5.6	
.....	.....	22	.....	.....	.....	.....	.....	68°	.....	95.20°	95.40°	+ .20°	97.60	100.90	+ 3.30	— 74.35	— 79 30	.....	4.6	
1	March 26.	16	00	.....	.....	.....	.....	65°	.....	102.50°	100.50°	— 2.00°	100 40	76.04	— 24.36	.....	.....	.....	4.8	
.....	March 26.	16	60[1]	5 minutes.	30 min.	100.50°	— 2.00	66°	45 minutes.	98.50°	91.90°	— 6.60°	90.28	11.08	— 79.20	— 10.12	— 64.96	3.75	.7	Dog perfectly paralyzed throughout the experiment.
2	March 31.	22	00	.....	.....	.....	.....	64°	.....	103.00°	102.60°	— .40°	139.08	132.48	— 6.6	.....	.....	00	6.0	
.....	March 31.	22	100[2]	11 minutes.	16 min.	102.00°	.....	67°	7 hr. 6 min.	102.90°	101.60°	— 1.30°	102.75	102.95	— 19.8	— 16.33	— 29.63	4.55	4.7	Twitchings of muscles on entering, stronger twitches and jerks on leaving, calorimeter; no co-ordinated movements.
3	April 2.	22.5	100[3]	9 minutes.	10 min.	102.70°	— 1.4	70°	6 hr. 47 min.	101.30°	98.90°	— 2.40°	115.90	75.40	— 40.5	— 23.18[4]	— 57.08[4]	4.55	3.4	
4	April 15.	15.5	00	.....	.....	.....	.....	71°	.....	102.70°	102 40°	— .30°	146.40	142.91	— 3.49	.....	.....	00	9.2	
.....	April 15.	15.5	22[5]	Few seconds.	1 min.	100.30°	— 2.7	72°	1 hr. 40 min.	{ 97.60°[6] 96.10° }	91.10°	— .50°	101.26	43.13	— 58 13	— 45.14	— 99 78	1.42	2.8	
5	May 5.	16.5	00	.....	.....	.....	.....	73°	.....	103.40°	103.10°	— .30°	136.64	131.93	— 4.71	.....	.....	00	8.0	
.....	May 5.	16.5	14[7]	?	8 min.[8]	102.40°	— .1	74°	1 hr. 53 min.	102.30°	101.55°	— .75°	102.74	133.45	— 9.29	+ 6.10	+ 1.52	.85	8.1	Twitchings just beginning to appear when dog was put into calorimeter.
6	May 9.	19.5	00	.....	.....	.....	+ .15	70°	.....	103.70°	103.80°	+ .10°	128.10	129.57	+ 1.47	.....	.....	00	6.6	
.....	May 9.	19.5	13[9]	.....	.....	103 20°	.....	72°	1 hr. 30 min.	103.35°	106.20°	+ 2.85°	119.34	152.02	+ 32.68	— 8.76	+ 22.45	.67	7.8	Dog perfectly paralyzed on entering calorimeter; could breathe and walk on leaving it.
7	May 10.	19	00	.....	.....	.....	.....	71°	.....	104.20°	104.20°	± 00°	117.12	117.12	± 00	.....	.....	00	6.2	
.....	May 10.	19	13	.....	8 min.	102.35°	+ .20	73°	35 minutes.	102.55°	104.00°	+ 1.45°	117.12	137.78	+ 20.66	± 00	+ 20.66	.68	7.3	Dog perfectly paralyzed on entering calorimeter; could breathe and walk on leaving it.
.....	May 10.	19	54	.....	.....	104.00°	— 2.4	73°	2 hr. 20 min.	101.60°	99.40°	— 2.20°	97.60	34.90	— 62.70	— 19.52	— 82.32	2.84	1.8	

8	May 12.	10.5	00	.....	.....	.....	.....	72°	.....	101.10°	98.70°	— 2.40°	91.50	72 60	— 18.90	.....	.....	00	6.9	
.....	May 12.	10.5	10.5	.....	.....	98.20°	+ .2	73°	38 minutes.	98.40°	96 20°	— 2.20°	98.82	81.49	— 17.33	+ 7.32	+ 9 29	1.00	7.8	Perfectly paralyzed on entering calorimeter; slight muscular twitchings on leaving.
9	May 18.	24.5	00	.....	.....	.....	.....	72°	.....	102.30°	101.80°	— .5°	254.98	245 75	— 9.19	.....	.....	00	10.0	
.....	May 18.	24.5	21[10]	2 minutes.	25 m.[11]	101.85°	— .15	74°	1 hour.	101.70°	103.50°	+ 1.8°	198.60	221.32	+ 22 72	— 56.38	— 24.47	.86	8.1	Dog came from under the influence of curare while in the calorimeter.
10	May 19.	14	00	.....	.....	.....	.....	72°	.....	102.30°	102.20°	— .1°	115 90	114.83	— 1.07	.....	.....	00	8.2	
.....	May 19.	14	16	.....	10 min.	102.6°	+ .1	74°	33 minutes.	102.70°	101.10°	— 1.6°	127.49	110.69	— 16.80	+ 11.59	— 4.14	1 43	7.9	Dog in calorimeter one hour and twenty minutes. Perfectly paralyzed on entering; showed slight incoördinated movements on leaving.
11	June 9.	21.5	00	.....	.....	.....	.....	74°	.....	103.35°	103.40°	+ .05°	196.42	197.23	+ .81	.....	.....	00	9.2	
.....	June 9.	21.5	8.5	3 minutes.	11 min.	103.6°	+ 2.4	77°	1 hr. 25 min.	105.00°	101 60°	— 3.40°	175.68	124.85	— 53.83	— 20.74	— 72.38	.4	5.8	Dog paralyzed on entering the calorimer; slight incoördinated movements on coming out.
12	June 10.	23	00	.....	.....	.....	.....	76°	.....	102.70°	101.10°	— .60°	138.64	128.14	— 14.50	.....	.....	00	5.6	
.....	June 10.	23	10.5	.....	.....	101.9°	— .50	76°	50 minutes.	101.40°	99 85°	— 1 55°	160.00	133.26	— 26.74	+ 21.36	+ 4.12	.46	5.8	Twitchings of abdominal muscles on leaving calorimeter.
.....	June 10.	23	25	.....	.....	99.85°	— .90	76°	25 minutes.	98.95°	97 65°	— 1.30°	71 68	49.25	— 22.43	— 66 96	— 98 86	1 09	2.1	
13	June 21.	29.5	00	.....	.....	.....	.....	81°	.....	102.90°	102.10°	— .80°	198.86	179.66	— 19.20	.....	.....	00	6 1	
.....	June 21.	29.5	8	.....	17 min.	103.45°	+ 1.35	83°	34 minutes.	104.80°	101.60°	— 3.20°	177.50	117.50	— 60.00	— 21.36	— 62.16	.27	4 0	Dog perfectly paralyzed both on entering and leaving the calorimeter.
.....	June 21.	29.5	27	.....	.....	100 80°	— .90	85°	14 minutes.	99 90°	100.30°	+ .4°	134.20	141.70	+ 7 5	— 64.66	— 37.96	.92	4.8	
14	June 23.	25	00	.....	.....	.....	.....	81°	.....	103.20°	102 60°	— .6°	152.5	141.25	— 11.25	.....	.....	00	5.7	
.....	June 23.	25	6[12]	.....	5 min.	101.1°	+ .45	80°	20 minutes.	103.55°	102 10°	— 1.45°	131.76	114.57	— 27.19	— 20.74	— 26.68	.24	4.6	Twitchings in all the muscles on leaving calorimeter.
.....	June 23.	25	75[13]	.....	.....	102.1°	— 1.75	80°	23 minutes.	100.35°	99 05°	— 1.30°	96 38	72.00	— 24.38	— 56.12	— 69.25	3.00	2.9	

[1] Curare injected subcutaneously. [2] Curare injected subcutaneously. [3] Curare injected subcutaneously.

[4] This was the same dog that served for the previous experiment (March 31). The numbers here are compared with his normal heat-production and -dissipation on that day.

[5] Curare injected into jugular vein in this and all following experiments.

[6] 97.6° was dog's temperature one hour and forty minutes after injecting the curare. There was a delay in adjusting the calorimeter, so that the dog was taken out and his temperature retaken. It was found to have fallen to 96.1°.

[7] Curare in two doses,—first dose, 5.5 milligrammes; second dose, 8.5 milligrammes; interval, five minutes. [8] After first dose.

[9] In three doses. [10] In two doses of 16 and 5 milligrammes respectively. [11] Time after first dose.

[12] Gave dog 13 milligrammes of curare three and a half hours previously, from which he had recovered for thirty minutes, when the present dose was given.

[13] 25 milligrammes per jugular and 50 milligrammes subcutaneously.

CATACAUSIS EBRIOSUS (*SPONTANEOUS COMBUSTION*).

By G. Archie Stockwell, M.D., F.Z.S., Port Huron, Michigan.

GENERALLY speaking, spontaneous combustion (so called) in the human subject, or indeed in any vertebrate form of life, is regarded with suspicion by the medical profession: First, because it is at variance with laws held to be firmly established; second, on account of its extreme rarity.

Of text-books of later date than Good's "Study," few condescend to admit the possibility thereof, works on medical jurisprudence excepted, while more exclude it altogether from the nosological catalogue. Not only have some of the most eminent authorities pronounced against catacausis, holding it to be the emanation of imaginative and sensational brains, and derived from the works of certain popular novelists, but many of our most expert physiologists and pathologists have been led to authoritatively deny its existence, as being a patho-physiological impossibility. Under such circumstances, it would seem an act of supererogation on the one hand, and superabundant egoism on the other, for the writer to either affirm or deny the existence of this supposed phenomenon. Hence, bearing this in mind, I will endeavor merely to lay the matter before the readers of the Gazette in its various phases, as derived from such evidence as is available in scientific literature, leaving them to draw such deductions as may (individually) seem just.

When we reflect that the human (or other animal) body, in every state with which we are acquainted, whether in health, disease, or after dissolution, is scarcely at all combustible; that it is not reduced to ashes save with the utmost difficulty, and then only with the greatest possible expenditure of heat,—as experience in this most ancient method of sepulture, and the history of those who have been condemned to the stake, abundantly testifies,—we cannot but be surprised that the general expression as regards *combustio spontanea humani* and *catacausis ebriosus* is overwhelmingly negative.

However, in spite of the difficulty of giving credit to so apparently marvellous an occurrence, examples of its supposed existence are so numerous, often so well authenticated, and come to us from so many different countries, sources, and widely-separated localities, that the evidences thereof cannot be wholly ignored or lightly brushed aside. "I confess," says that most eminent writer, M. Pierre Aime Lair,* "that these accounts appear to me to be worthy of very little credit; but they are presented by men whose veracity is unimpeachable,—men distinguished for their learning. Besides, it is not more surprising to meet with such incineration, than a discharge of saccharine fluid from the body, or an appearance of bone softened to a state of jelly."

Dr. Apjohn, of Dublin, with great succinctness and exceeding minuteness, gives the history of four cases of catacausis that occurred in Ireland prior to the year 1830,† of which the following, condensed, is the most illustrative:

A woman, about 60 years of age, residing with her brother in County Down, retired one evening with her daughter, both being (as was their constant habit) in a beastly state of intoxication. A little before daybreak, the members of the family were awakened by smoke and an extremely offensive odor, which pervaded the entire dwelling. These were traced to the apartment occupied by the old woman and her daughter, and found to proceed from the body of the former, which, to employ the language of her brother, as given at the coroner's inquest, "appeared to be burning up with an internal fire." The trunk of the unfortunate woman was further described as "black as a coal, with smoke issuing from every part of the chest." With the greatest difficulty combustion was finally arrested (though no evidence of flame was at any time apparent), when, of course, life was found to be extinct. "While the corpse was being removed into a coffin, which was done as soon as possible, it was continually dropping in pieces."

The daughter who slept in the same bed sustained no injury; neither did the fire extend to the couch or clothing, which exhibited no evidence of the catastrophe other than stains due to smoke and grease. The universal testimony of relatives precluded the possibility of ignition from other than spontaneous causes, as fire and lights were carefully extinguished each night on retiring, solely on account of the habits of the deceased, who had been grossly intemperate for years, and more especially so for some weeks immediately preceding the accident.

MM. Dupuytren and Marc published in detail the history of two cases which they personally had opportunity of observing.

* Jour. de Physique, Ant. viii.

† Cyclopædia of Prac. Med.

MM. Kopp and Lair collated twenty-five cases, deemed by them authentic, of which twenty-four were females and confirmed gin-drinkers. M. Devonald in 1833* chronicled a case occurring in London, which is believed to have afforded material for the tragic death of the mother of Jacob Faithful, as depicted by Captain Marryatt in his novel of that title,† and also for that of Old Krook, as described in "Bleak House" by Charles Dickens.‡

Three years later,—viz., 1836,—in the Commune de Surville, near Pont L'Eveque, occurred the most notable and definite case yet chronicled,§ aside from that of Dr. Apjohn, as already cited, and which involved the lives of two individuals.

On the 7th of September the Procureur du Roi was notified that an old man, of 70, and his wife, ten years younger, both of whom had been long addicted to the immoderate use of spirituous liquors, were supposed to have met with an accident, since their dwelling had been closed for several hours, and they had failed to appear at their usual time or to respond to the repeated calls of neighbors. Further, the information was volunteered that both, when last seen the evening before, were in a horrible state of ebriety, and that a most sickening odor perceived in the locality appeared to proceed from the house by them occupied.

On entering the dwelling a strong empyreumatic odor was observed, and in the principal room, the door, its latch and casings, and the furniture, walls, and ceiling, were found covered with a peculiar grayish, sticky, unctuous, and offensive soot. On opening the shutters this was still more apparent on the windows, marble of the fireplace, and the glasses that chanced to be within the room. And on the floor was discovered a shapeless carbonized mass, in which nothing definable could be recognized but four human legs.

Two of these limbs were covered with black woollen hose (it is somewhat remarkble the degree to which woollens resist the action of heat and flame, and that the flesh beneath may be burned without in the least injuring the texture of the fabric) and prunello gaiters. One of the stockings was very slightly charred at its upper extremity, though the subjacent tissues exhibited no unusual appearance, except that portions of epidermis protected by the boots were slightly reddened. Three inches above the knee, however, the thighs were converted into a black, shapeless, carbonized mass; and except a small portion of the superior edge of the left ilium, nothing could be recognized of the inferior portion of the trunk, which had separated from the superior at the junction of the sacral and lumbar vertebræ. The appearances indicated that this corpse had laid at right angles or nearly so across the one below, the weight of the head and upper extremities causing it to fall forward and separate. The two or three vertebræ which, in consequence, had become exposed to the air, were completely calcined.

The whiteness of the vertebral bones contrasted singularly with the dark, pitchy, carbonaceous mass into which the chest and its contents were converted. The ribs and sternum were completely destroyed, with the exception of the vertebral ends of the first two of the former on the left side, which, though much blackened and charred, still retained their articulations. The bones of the neck (cervical vertebræ) were completely calcined, and terminated superiorly in a white, rounded, and incinerated mass which had formerly been the head. Of this the skull was so friable that, with an attempt to lift it crumbled into powder, leaving it and the brain a mass of ashes, the lower jaw, which rested upon the floor, alone retaining its shape.

Under the remains of this corpse, as already noted, lay the débris of a second. Of this the left lower extremity was bare, and spotted throughout its anterior surface with vesicles filled with reddish serum, the contents of which upon exposure to air were immediately dissipated in a flash of blue flame; but, posteriorly, along the whole extent, the leg was burned and scorched to the very bone, the latter being half calcined. It was almost quite detached from the pelvis, and exuded at the upper part a most hideously fetid oleaginous fluid. The right leg, partially flexed upon the thigh, rested with the knee upward, the sole of the foot pressing the ground. The ankle-joint was infiltrated with dark serous fluid, but the joint of the knee was laid quite open and completely desiccated. Also the thigh-bone had snapped across at the junction of its lower and middle thirds.

From just above the fractured point on the right limb, both thighs were converted into a confused mass of unctuous charcoal; and the pelvis and contents, as well as the trunk superimposed, were entirely consumed. It was immediately above the pelvis of the second corpse that it was crossed by the first; and it was by

* British Med. Jour., 1836.
† First published in 1838.
‡ First published in 1854.
§ Jour. des Connais. Méd.-Chirurg., September, 1836.

no means easy to separate the adhesions that existed between the two, owing to the slow and stifled combustion that must have taken place, and that, so to speak, welded the two together.

The liver and right lung of the under body presented comparatively natural appearances, and were easily recognized, having been well protected by the carbonized layers of clothing, and the superadded mass of the other corpse, but they had shrunk to about half their normal size, presenting hard, brittle, shining surfaces, which, under the scalpel, exhibited the consistency of cheese. Spine and ribs were alike reduced to charcoal of most solid consistence, being much more firm than that resulting from combustion of the soft parts. The head of this corpse was entire, but so besmeared with sooty and unctuous matters, and so generally disfigured, that the features were unrecognizable: and though the projection of the nose and hollows of the orbits were readily definable, they disappeared,—the bones giving way and crumbling into numerous pieces,—the moment an attempt was made to lift from the floor. The brain, "shrivelled and hard as a piece of horn, and not larger than a hen's egg, rested upon the rim of the spinal opening at the base of the skull."

Of the upper extremities of both bodies, the only remains were a calcined portion, some three or four inches in length, of the bone of one arm, and a few bones (metacarpal) of the hands; and even these were quite incinerated. Exclusive of the lower extremities, which were much less reduced than the upper parts, the entire weight of the débris of both bodies did not equal *four pounds!*

The floor on which the bodies lay was soiled with the same unctuous, sticky, and stinking substances that pervaded the ceiling and walls, and the débris of the two corpses was surrounded by the following objects: a table, close to the feet that were farthest from the fireplace, not in the least scorched or injured save only as it was covered with the unctuous substances already mentioned,—this held a bottle and two glasses, all containing *eau-de-vie;* a fireplace, along with a high fender filled with ashes, close to the two heads; a gridiron, which had fallen beneath the body of the under corpse (presumed to be that of the woman); and a brand of wood, still ignited ("*un tison encore enflamme*"), lying between the two heads. Beside these, a single shoe lay to the right of the bodies; and to the left, between the wall and the lower extremities of the remains supposed to be those of the man, stood a chair, very slightly scorched as to varnish.

The period in which the combustion had taken place, counting from the time the unfortunates were last spoken up to the forcing of the door by the authorities, could not have exceeded twelve, or at most fourteen, hours!

With regard to the non-appearance of *true* spontaneous combustion in the human subject of late years, no one is in condition to speak. Certain it is, the consumption of alcoholic beverages has not decreased to any appreciable extent, but, on the contrary, in some portions of the globe has greatly *in*creased. Catacausis as the sequel to ignition, however, has been observed in a few isolated instances, the last of which I can find any record (one of a series of three, which will be cited further on) occurring in 1887.

Strange to say, too, peculiar accidents appear to be epidemic, as it were, several following in rapid succession, though through what psychical laws or phenomena of coincidence they are brought about, is entirely beyond the human understanding. For instance, the vulgar hold that after a long period of immunity, one conflagration will be followed by a second and a third, ere another period of immunity occurs, and observation gives fair confirmation; and the theory was recently well exemplified in Michigan, where a totally unheard-of accident was followed in quick succession by two more of precisely the same character.

The question as to the spontaneity of human combustion admits of no little argument, in spite of the fact that certain works on jurisprudence have affirmed it. It seems to be a well-authenticated fact, however, that bodies have in some strange manner become ignited, and partially or almost wholly consumed, leaving only a mass of charcoal, ashes, and incinerated grease. Some of these cases have been traced to stoves or open fires, some to lamps, candles, and pipes, others to strokes of electricity or lightning;* but a few, in spite of all study and research, no matter how thoroughly or carefully conducted, elude all effort to elucidate a cause; and it is these last that have given rise to the theory of spontaneous development. In no case, so far as known or reported, has the entire body been destroyed, some portion or portions being merely scorched, while of the residue nothing remained but a few ashes, or a trifle of charcoal mixed with black unctuous matter. And that there must have been something more than mere contact with flame

* Foquet, *Jour. de Méd.*, tome lxviii.

or fire, is apparent from the fact that the thoroughness and completeness of the reduction of animal tissues and matters is unparalleled, and that the heat, though so powerful as to consume the most incombustible portions of the body, is yet almost or wholly devoid of flame, and apparently powerless so far as ordinary substances are concerned, since it is resisted by wood, clothing, and other inflammable matters, even though brought in direct contact therewith. Moreover, it must be observed that the victims are invariably stricken down suddenly and without warning, while those of ordinary accidents by fire rush hither and thither in their frenzy, falling only when completely exhausted or overpowered.

The peculiar chemical state of the body that could induce a catacausis, to say the least, is both singular and mysterious. Dr. Thomas Younge,* I believe, was the first to embody catacausis in a medical treatise, and attempt a scientific explanation thereof; and he also created for it a new genus in medical nosology, derived from *κατακαιω*, "I burn," from which Dr. Good† formulated the specific title of *catacausis inebriosus*, which he defines as a "constitution inflammable in consequence of long and immoderate indulgence in spirituous liquors: the combustion *easily excited or spontaneous.*"

The bodies of habitual topers have not, as a rule, however, been found especially combustible; neither do flesh or morbid specimens, even after long saturation in alcohol, exhibit any such property, but rather the reverse,—the alcohol alone is consumed, the specimen being charred upon its exterior merely. For these reasons it is evident that an inebriate catacausis must be due to chemical changes solely, taking place within the economy or tissues, though exactly of what such changes must consist, is as yet beyond the grasp of science.

In the majority of instances the clothing of the victim is destroyed (though in a few it is but little damaged, or at most slightly scorched or charred) where it is immediately contiguous to the parts of the body most involved. But the thick, greasy, offensive soot evolved permeates everything, covering walls, ceilings, and furniture, even making its way through the joints of doors, windows, and casements, into adjoining apartments.

On a few occasions the human body, if we may believe the testimony of veracious witnesses, has been discovered in the very act of combustion, a bluish flickering flame being emitted; and we are assured by all authorities and observers, that water, far from quenching, tends to add fuel to the fire.

From the proportion of cases which have met death while in close proximity to fire or flame, it seems more than probable that ignition in a majority of instances did not take place spontaneously. In one case, as detailed by Dr. Apjohn, combustion occurred apparently as a sequel to an attempt to light a pipe with a live coal, the flame being described by an eye-witness as "bursting out from the throat and bosom all at once,"‡ the woman "falling instantly, as if felled by a bludgeon."

Several cases, however, cannot be thus satisfactorily accounted for. In that first cited in this paper, the sworn testimony of the inquest evolved the fact that neither light nor fire existed in the dwelling at the time of the accident, and that, moreover, the "stupid condition of the daughter, not less intoxicated than the mother, indicated that neither were in condition to search for, much less strike, a fire." This, of all cases I have been able to discover by extensive research and examination of medical and other scientific literature, presents the most reasonable and convincing evidences of being a *bona fide* instance of spontaneous combustion in the human subject.

M. Duvergie,§ from personal study and observation, unhesitatingly declares that spontaneous catacausis is not only possible, but probable; and he suggests that the modification in the human economy permitting such phenomenon, is due to the action of the forces in the living body rendering the tissues combustible by absorbing a large amount of alcohol, or by transforming alcohol into still more inflammable compounds. In defence of his position, apparently, and to prove that pathochemical changes may take place that favor inflammability of animal tissues, he cites two well-authenticated cases,‖—one of a young man, the other a girl scarce out of her teens, both perfectly healthy seemingly, not possessed of unusual obesity, and *strictly temperate* in habits, who, with no premonitory symptoms of disease, had flame communicated to the ends of the fingers, setting fire to the clothes that enveloped them; and though im-

* Younge's "Medical Literature," 1813.

† Good's "Study," vol. iii., 1829.

‡ Alexandré, "De Spontanea Humani Conflagratione," 1830.

§ Philosophical Transactions, vol. xiii.

‖ Philosophical Transactions, vol. xvi.

mersion in water extinguished the fire, it broke out spontaneously the moment the hands were dried. The effect did not extend beyond the hands, and, the flames being smothered, the burns healed readily and kindly, with no other loss than of a slight amount of epidermis and one or two nails.

The most modern case of supposed catacausis that I have been able to resurrect is that reported by Dr. George O. Williams, of Greene, Chenango County, N. Y., occurring in a youth, of 18 years, as a sequel to a gunshot wound. It is reported as follows:*

"He had dinner at noon. The body was found at 2 P.M. lying on the dry plank floor of the stable. The clothing inventoried one pair of cotton trousers, one overalls, one—possibly two—cotton shirts, and shoes and stockings; estimated weight, exclusive of shoes, four pounds. All the clothing was consumed, except fragments on the ankles and one shoulder. These were still burning, but without flame, when the body was discovered. An edge of the plank on which the body lay was charred for six inches, and at one point a small flame appeared. The surface of the plank beneath the body was slightly browned for a diameter of twelve inches. The skin, even on the face, was deeply burned,—cooked,—and fully one-half of the surface of the body was deeply browned in detached areas. On these areas were gaping fissures in the skin, four to twelve inches long, exposing wasted fat, and in some places muscular tissue. The fissures in the flexures of the thighs and knees were deep, and the exposed tissues tender. The genitals were charred, the foreskin crumbly. On making a section, the tissues felt, under the knife, and looked, like any roasted flesh, to some extent even on the internal thoracic walls. A cartridge-ball had traversed the upper and lower lobes of the left lung, and the right auricle and left ventricle of the heart. Coagula filled the thorax; the lungs were collapsed. He had taken whiskey during the forenoon, quantity unknown; and his habits were reported to have been somewhat unsteady. The query arose, Why should the body have been so deeply and extensively cooked with the limited amount of fuel (four pounds of cotton clothing) and at the same time the dry planking beneath so trivially damaged?"

Doubtless many readers of the GAZETTE have encountered topers whose pulmonary exhalations, on coming in contact with flame, gave a luminous effect,—produced scintillations of blue light. Several such cases are recorded in medical literature, and would seem to have more or less bearing upon the history and phenomena of catacausis. One of the most notable of these † occurred in the almshouse near Liverpool, England, on the evening of November 16, 1844, when, for twenty minutes previous to death, a patient emitted luminous breath compared by the attendants to a "red-hot coal-like streak." The unfortunate, some fifty years of age, and who had been denied spirituous liquors for forty days, suffered from dropsy as a sequel to chronic alcoholism, complicated with hemorrhage from bowels, cough, and expectoration. The phenomenon was first observed by the male nurse in the ward, who called the attention of others to the "red-hot appearance of the dying man's cheek." There was no possibility it could have been caused by reflection from the fire in the fireplace, or from the burning gas-jet, both of which were very low; and, unfortunately, the lookers-on regarded the matter from a moral rather than scientific stand-point, and did not mention it to the attending surgeon until long after interment, when it was too late for post-mortem examination.

A parallel case is reported by Dr. William Huggins, of Trinidad, West Indies.‡ On the night of August 7, 1844, the black, who was nursing a habitual toper, prostrated with diarrhœa and dyspnœa resultant upon excesses, called the overseer of the estate (an American Quaker of undoubted veracity), stating the patient was rapidly approaching dissolution. The latter arrived just in time to see the man die, and plainly observed a flame of bright color issuing from his mouth, and immediately disappear. Says Dr. Huggins: "When he told me of it the following day I laughed at him, saying he must have seen a reflection from the candle in a bubble of saliva. He replied, it could not have been so, from the position of the candle; and on my inquiring of the black attendant I found that he also had seen what he termed 'fire come out of the man's mouth.'"

Doubtless, too, some of my readers may have encountered the bloated and besotted specimen of humanity that during the summer of 1877 perambulated the streets of New York and Brooklyn, exhibiting himself in bar-rooms and restaurants as possessed of

* Med. Record, August 13, 1887, p. 183.

† Transactions Liverpool Pathological Society, 1845.

‡ Lancet, September 6, 1845.

a luminous and inflammable breath. Armed with a small tin dish, the bottom of brass strainer-cloth, he would respire into the flame of a taper or gas-jet, thereby procuring sparkling scintillations of blue light. This was his means of securing the desired libations of the ardent; and it may be that his subsequent demise by drowning saved him from a more horrible fate.

MM. Bianchini, Raffei, Rollin Le Chat, Vicq d'Azyr, Kopp, Marc, and other gentlemen, equally distinguished for learning and physiological and pathological research, from the histories evolved, unhesitatingly advance the opinion that ignition of the human body does not always depend upon the application or approach of a burning object, but that it may, and does, develop primarily and spontaneously in the economy itself.*

Apropos of this, it is well known that many inorganic and non-inflammable objects, under certain circumstances and surroundings, will, of themselves, become heated and inflame. Combustion has even been known to occur spontaneously in heaps of ashes that had not been added to or in any way disturbed for months or years.† Again, many chemical combinations will cause ignition, either immediately or by continued exposure to the oxygen of the atmosphere: thus, when nitric and sulphuric acids are together brought in contact with an essential oil, the whole at once bursts into flame. It is upon such and similar facts that M. Bianchini *et al.* base their belief that animal bodies, in either life or death, may through physiologico-chemical changes spontaneously ignite.

The development of the electrical spark on the one hand, and the imbibition of all the tissues of the economy with spirituous and inflammable liquids on the other, especially when at the same time there is an abundant deposit of adipose in the cellular substance, are circumstances which many physiologists have considered as all-sufficient to procure and feed a catacausis. It is well known to pathologists, also, that alcoholic beverages, employed to excess, may be quickly absorbed into the blood, and thus speedily diffused throughout every part of the body. For example, MM. Cuvier and Dumeril were struck with the strong alcoholic odor exhaled from every part of the corpse of a man who met his death by swallowing an extraordinary amount of brandy in compliance with the terms of a wager.‡ Again, M. Breschet mentions observing the same phenomenon in a criminal after execution, whose last moments had been gratified by repeated libations of spirits; and he adds the important fact that "fat animal substances are always found to burn much more quickly than lean, and still more quickly if saturated with alcohol." He observed this frequently in the anatomical theatres of Paris, when he wished to speedily get rid of the débris of the dissecting-table.§

M. Marc concluded, from numerous observations and experiments, that animal bodies may engender and become impregnated with inflammable gaseous matters, and that these may ignite, either upon the approach of any lighted body or by the action of an internal electrical current; and a case is by him cited, occurring in the Hôtel Dieu, which in a measure corroborates the first portion of this hypothesis:‖

The body of a male patient, that died suddenly from obscure disease, was held for autopsy, but owing to preoccupation of the hospital staff, was not examined until the eighth day, when it presented unmistakable evidences of decomposition. The whole surface of the corpse was swollen and bloated from distension of cellular tissue, and covered with vesicles containing either reddish serum, or gas. The cavities of the abdomen, thorax, and also the ventricles of the brain, were found distended with the same gas that filled the vesicles, presumably carburetted hydrogen since it caught fire and burned with bluish flame on the approach of a candle.

With such evidence before us, and knowing that decomposition to considerable extent may take place before life is extinct, the theory of spontaneous combustion in the human or animal subject seems less problematical. Moreover, it is not at all improbable that where carburetted hydrogen is developed, it may be replaced on occasions by the still more inflammable phosphuretted gas, which ignites upon mere contact with atmospheric air: there is absolutely nothing in the chemical combination and construction of the physical economy that is inimical to such evolvement. The *ignis fatuus*, "will-o'-the-wisp," and so-called "elf-candles" are supposed to owe their origin to phosphuretted hydrogen, and they are

* *Surgical Review*, Ploncquet, "Littérat. Méd.;" Dupont, "De Corporis Humani Incendies Spontaneis."

† American Jour. Med. Sciences; Am. Cyclopædia.

‡ Robie on Alcohol.

§ Gazette Médicale (Dijon), 1844.

‖ Bartholin, Act. Hafon., i., Obs. 113.

found only in neighborhoods where decompositions of animal and vegetable matters are constantly occurring, such as cemeteries, marshes, dung-heaps, and low wet grounds. In Oriental and tropical countries where interment is practised, they are most frequently observed in and about tombs and burial-places, especially where bodies are inhumed at shallow depths and the torrid heat of the sun has baked the surface, causing the ground to crack and gape, allowing free passage for the gaseous products of decomposition. Neither have we any authority for denying that phosphuretted gas may be developed as a sequel to chemical change within the living body, for the elements essential thereto are never absent: and in such case, if oxygen be present, the mixture necessarily assumes an explosive character adequate to effect the rapid combustion affirmed of spontaneous catacausis. It may likewise be remembered that "elf-candles" and "will-o'-the-wisps" are unable to ignite ordinary substances, and that the combustion of human bodies that has been deemed spontaneous has proved equally inert!

The medico-legal aspects of catacausis, whether spontaneous or communicated, are particularly interesting: First, to quote Stillé and Wharton, because, "In approaching the examination of the question, whether such a phenomenon is really possible, we encounter the usual obstacle to the discovery of the truth, viz., a doubt as to the authenticity of the fact upon which the belief reposes." These authors quote three* cases, none of which, however, are as satisfactorily substantiated as those already presented in this paper, and which appear to be unknown or altogether overlooked by jurists. Second, the general doubt that must exist as to spontaneity. Third, the incompetence of the majority of witnesses, who are of the laity and biased by both superstition and excitement.

However, accepting the evidence in the main as true, one strange fact remains,—that, of the seventy-two cases of spontaneous combustion and catacausis that are chronicled in medical literature (some dating back more than two centuries), the majority are women in advanced life, addicted to the immoderate use of *gin*,† a liquor largely imbued with empyreumatic oils. This may be further accounted for by the tendency of the sex to the accumulation of adipose, and that drunkenness when indulged in by women is usually carried to greater excesses than by men, which is to be expected from the more exquisitely sensitive nervous organization of the former.

So far as I have been able to discover, the claims of spontaneous combustion have not demanded the services of experts in courts of justice above three times. One of these cases will be remembered by many of the older readers of the GAZETTE. I refer to the trial of John Staff for the murder of the Countess of Garlitz at Darmstadt,‡ the defence being spontaneous catacausis, the body of the unfortunate lady having been partially consumed by fire. Ingenious arguments were advanced by many of the most learned of the legal profession to convince the court that this, and this alone, was the cause of death, and with such success that the trial was carried over some months. Meantime, the body of the lady was exhumed and examined by experts, who detected a fracture of the skull and other evidences of murderous assault that must have been received prior to the action of fire; moreover, the thorax had suffered little, the extremities and the head being the parts consumed.

January 31, 1889.

A BACTERIOLOGICAL MANIPULATING CHAMBER.

BY SAML. G. DIXON, M.D.§

THE apparatus about to be described is adapted to retain to a reasonable degree a sterilized working space during the manipulations requisite for the cultivations, etc., of bacteria.

To use the manipulating chamber—for example, to inoculate a number of test-tubes—the operator proceeds as follows: The tubes to be inoculated, filled with the requisite quantity of nutrient gelatin or agar-agar and plugged with sterilized cotton, are placed in the chamber in wire baskets or on the rack (No. 3), together with needles, slides, cover-glasses, etc., and the lids (No. 5) securely clamped over the apertures *a*. The ventilating aperture at the top of the chamber should be filled with cotton to allow the free exit and entrance of uncontaminated air as the apparatus is heated and cooled. Or, if desired, a small thermometer may be intro-

* Pages 420, 421.

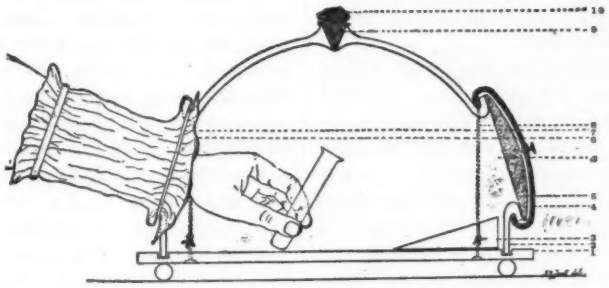
† London Medical Gazette, 1850.

‡ Stillé and Wharton, "Medical Jurisprudence."

§ Professor of Hygiene, University of Pennsylvania, Phila., Pa.

duced through this opening, and the cotton packed around it. The whole is then sterilized in a hot-air sterilizer, taking care to have the temperature in the interior of the chamber sufficiently high to destroy all spores as well as bacteria, or else to reheat the chamber on two or more days.

After cooling, and immediately upon the removal of the apparatus from the hot-air sterilizer, the cords (No. 8) are passed over the necks of the apertures (*a*), and fastened to the bed-plate to prevent the cover from being accidentally lifted from the latter at any time during the manipulations. If neces-



No. 1 represents an iron plate, grooved on top to receive No. 2.

No. 2. A round glass cover, sixteen inches in diameter and ten inches high.

No. 3. A rack for instruments, test-tubes, etc., preferably made of glass.

a. A round aperture about four inches in diameter. (The lips at the margin of this opening and of its fellow opposite must be ground perfectly flat.)

No. 4. Iron clamp with thumb-screw, to hold in place No. 5.

No. 5. A glass cap or lid made with one flat ground surface to fit on ground surface of lips around the aperture *a*.

No. 6. Thin rubber sleeves twelve inches long and eight inches in diameter.

No. 7. Thick rubber bands.

No. 8. A cord stretched over the neck of "*a*" and hooked down to the bed-plate (No. 1).

No. 9. An aperture to receive a thermometer or No. "10."

No. 10. A cone-shaped sterilized cotton plug.

sary, a little oil or paraffine can be poured into the groove in the bed-plate.

The rubber sleeves (No. 6) are thoroughly washed, inside and out, first in soap and water, then in bichloride of mercury solution, and, while still wet with the latter, are slipped over the hands and arms of the operator, these also having been thoroughly washed and disinfected, until the fingers reach to the farther ends of the sleeves. Great care must be observed not to admit air into the sleeves. The thick rubber bands favor this exclusion of outside air by closely adapting themselves to the varying contour of the fingers, hands, and arms.

The right-hand glass cap is then washed and disinfected, the lower end of the corresponding sleeve is firmly pressed against this cap, and the fingers stretch the band over the lips of the aperture and allow it to contract around the neck of the same. Then the clamp is unscrewed, and lid and clamp are permitted to drop into the sleeve beside the operator's arm. The hand has now free access to the sterilized chamber. The left hand is conducted into the chamber in a like manner, carrying with it the tube containing the culture for inoculation. In order that this last tube may be thoroughly sterile as to its exterior, a well-washed and disinfected rubber cap (an ordinary finger-stall) is slipped over the cotton plug of the tube, and the tube then washed in the bichloride solution. Now, when the rubber cap and cotton plug are removed from the tube in the chamber, the cap, by contracting upon and enclosing the plug, will prevent any germs that may have been entangled in the cotton from escaping into the atmosphere of the chamber.

After both hands of the operator are in the chamber, they can be dried, if necessary, on a little absorbent cotton, placed for the purpose on the floor of the chamber. The cap and plug are then slipped from the tube containing the growth, the sterilized tubes taken up one after another, inoculated, plugged, and laid aside, slides prepared and cover-glasses adjusted, just as is ordinarily done in the laboratory.

When all the desired work is accomplished, the hands are removed, the cords unfastened, the glass cover lifted from the bed-plate, and

the inoculated tubes and slides stored away or placed in an incubator, as the operator desires.

In the same manner plate cultivations may be made. In fact, all chambers for plate or potato cultivation should be made on this or a similar plan, inasmuch as the large area of the plates or potatoes greatly increases the danger of contamination while they are being prepared and inoculated.

Cover-glass impressions may be made from time to time from the same plate. Or, with such an apparatus, a microscope may be inserted through one of the arm-apertures, the objective and eye-piece removed, a cotton plug placed in the draw-tube, and the space around the microscope in the aperture packed with cotton, the whole sterilized by heating, a plate cultivation made on a slide, the objective washed in a solution of carbolic and lactic acid and applied, the eye-piece inserted, and the same growth observed from time to time. Yes, it may even be photographed at any period of its development, or many times, by means of the appliance for making photo-micrographs with the microscope in the upright position devised by Dr. Seneca Egbert, of this city, and described by him in *The Microscope* for October, 1888.

Again, small organs or animals may be dissected, living ones inoculated, and any of the ordinary bacteriological operations performed with the minimum of danger of contamination.

The above manipulations may seem tedious or complicated, but are in reality very simple, and will take far less time than is here necessary to describe them. And, if we are to believe that there are micro-organisms, many or few, contaminating the air of our laboratories, the use of such appliances is certainly a step towards scientific accuracy, and the results thus obtained must be *the more valuable* as they are *the more accurate.*

---

*THREE CASES OF VALVULAR INSUFFICIENCY.**

By W. G. Kemper, M.D., Omaha, Neb.

My attention was first called to this subject by the following case: A pauper, 64 years of age, suffered from frequent attacks of shortness of breath. He was often examined by different physicians in the neighborhood, all of whom considered the case an excellent one illustrative of organic mitral disease. A murmur, distinctly audible by the unaided ear a few inches from the chest, replaced the first sound, and was loudest at the apex. Percussion-dulness was increased towards the left. The pulse was frequent, weak, and irregular. The lungs showed symptoms of œdema. Liver and spleen were enlarged. The urine was not albuminous. The man had worked hard in his earlier years, but at this time he did such work as he felt able. He had never had rheumatism, if an occasional lumbago was excepted. Many remedies, among which I remember digitalis, convallaria, and nitro-glycerin, were tried, without success. The patient's condition remained about the same, until eventually he died of an inflammatory bowel-disease; and at a post-mortem held the following day this condition was found: The left side of the heart was considerably dilated and hypertrophied; the right side slightly so. The mitral valve was not diseased, but the ostium was enlarged to such an extent that the valves were insufficient by about a line to prevent regurgitation. The kidneys were healthy. I believe that cases of this kind are more common than is generally supposed, particularly among the aged, those who have been obliged to do hard labor and were unable to obtain good food. The symptoms came on very gradually, with no history of rheumatism. It is doubtful whether patients of this kind can be improved by treatment. In the younger cases Oertel's treatment might be of benefit, if it could be carried out. But it is *positively* known to be of benefit *only* in those cases which owe their origin to a want of proper exercise. A heart-tonic is indicated; digitalis is probably the best.

The second case is that of a farmer, 17 years of age. He had done very hard labor on the farm during the year, and was complaining when I saw him of dyspnœa and palpitation, which became worse with every muscular effort. His face was cyanotic and pale; his lower limbs œdematous. Percussion-dulness extended about one-fourth inch to the right of the sternum, and an equal distance to the left of the left mammillary line. A loud systolic murmur at the apex was followed by a feeble second sound. At the base the sounds were weak but clear. The lungs showed symptoms of œdema. The pulse was almost imperceptible. There was no history of rheumatism, and his family was healthy.

* Read before the Omaha Academy of Medicine, February 5, 1889.

He had noticed symptoms of weakness about four months. He was advised to go to bed, and digitalis with small doses of iron and good nourishment were prescribed. Under this treatment he recovered very rapidly; and a month later I could not find the slightest symptom of heart-disease. In this case the large area of dulness and feeble heart-sounds pointed to dilatation of both ventricles, to which evidently the mitral insufficiency was due.

The third case is that of a young married lady, to whom I was called on November 9, 1888. She had the evening before attended a party, at which dancing was the principal amusement. She danced often, but was obliged to discontinue the exercise on account of breathlessness, which discomfort troubled her until she had returned home and had fallen asleep. She was accustomed to dancing, but in this case the dancers used a canvas instead of a waxed floor. This she thought was the cause of her fatigue. On arising the next morning she had a succession of fainting fits, and was obliged to return to bed. Her pulse was 105, at times weak and frequent, then full and very slow. In four consecutive quarter-minutes I counted 23, 30, 24, and 28 beats. She had no fever. Per auscultation of the heart I discovered a blowing murmur, with the first sound at the apex. At the base the sounds were normal. The heart-dulness was not increased. Her occupation was light. She had always been healthy, and had never suffered from heart-symptoms previous to this attack. The patient remained in bed for three days, during the first two of which she took 2-grain doses of bromide of caffeine, after that 10-drop doses of fluid extract of digitalis. On the third day her pulse was 86, but regular; the blowing murmur was very faint. Two days later the heart-sounds were normal; the pulse 80. Since that time I have examined her twice; but at neither time was I able to detect a symptom of heart-disease. This case was peculiar. There was mitral insufficiency, but I found no signs of dilatation. Nevertheless tight-lacing and fatiguing exercise may have produced a relaxation, which temporarily increased that normal insufficiency, which many physiologists claim is often present with no other symptoms in health.

In conclusion I would ask this question, May not relaxation of the chordæ tendineæ and papillary muscles be a cause of regurgitation?

1524 South Seventeenth Street.

ALKALOIDS OF THE ARECA-NUT.

The areca-nut, as it is known, is extensively used as a masticatory by Asiatics, and comparatively recently certain statements made as to its virtues as a vermifuge won for it a temporary place in the British Pharmacopœia. According to "Pharmacographia" its principal constituents are crystalline fat and red amorphous tannic matter; but about three years ago Herr Bombelon announced that he had separated from the nut by means of ether a volatile liquid alkaloid that he named "arekane" (*Pharm. Journ.* (3), xvi. 838). The nut has been further investigated by Herr Jahns, who reports that he has separated from it three alkaloids by precipitation of an acidulated water-extract with iodide of potassium and bismuth and sulphuric acid and suitable decomposition of the precipitate (*Berichte*, xxi. 3405). "Arecoline," the most important of the three, is described as a colorless, oily liquid, having a strong alkaline reaction, soluble in all proportions in water, alcohol, ether, and chloroform, and distilling at about 220° C. Its composition corresponds to the formula $C_8H_{13}NO_2$, and it forms salts that are mostly crystalline, readily soluble and hygroscopic, but the hydrobromide is stable and non-hygroscopic. Preliminary physiological experiments have shown this alkaloid to be poisonous, and to be probably the active constituent of the areca-nut, to which its reputation as a vermifuge is due. In this respect, as well as in its chemical and physical properties, arecoline appears to approximate to pelletierine, the alkaloid of pomegranate-root bark, which is represented by the formula $C_8H_{15}NO$. "Arecaine" ($C_7H_{11}NO_2 + H_2O$), the second alkaloid, forms stable colorless crystals, readily soluble in water and dilute alcohol, less soluble in stronger, and almost insoluble in absolute alcohol, and insoluble in ether, chloroform, and benzol. An aqueous solution is neutral in reaction, and has a faintly saline taste. Arecaine combines with acids to form crystalline salts having an acid reaction and soluble in water. In its properties it appears to stand near to trigonelline, the alkaloid of fenugreek-seeds (*Pharm. Journ.* (3), xvi. 447), and, like it, appears to be physiologically inactive. The third alkaloid, as obtained, was amorphous, strongly alkaline, readily soluble in water, alcohol, and chloroform, and with difficulty in ether, but was in too small quantity to be investigated. —*Pharmaceutical Journal and Transactions*, February 2, 1889.

The Therapeutic Gazette

EDITED BY
HORATIO C. WOOD, M.D.,
AND
ROBERT MEADE SMITH, M.D.,
PHILADELPHIA.

GEO. S. DAVIS,
Medical Publisher, Box 470,
DETROIT, MICH.
Philadelphia, 714 Filbert Street.

SUBSCRIPTION RATES FOR 1889.

(Journal increased to 72 pages.)

THERAPEUTIC GAZETTE (postage included).....$2.00
THERAPEUTIC GAZETTE with MEDICAL AGE..... 2.50
THERAPEUTIC GAZETTE with AMERICAN LANCET 3 25
THERAPEUTIC GAZETTE with AGE and LANCET... 4.00

Foreign subscriptions may be ordered through our agent in England, Mr. H. K. Lewis, Medical Publisher and Bookseller, 136 Gower Street, London. Price 10s. Remittances may be made either by Postal Order or Stamps.

Price to Foreign Subscribers direct (postage included) $2.50 (10 shillings). English postage stamps received on remittances.

Editorial communications should be addressed 1925 Chestnut Street, Philadelphia. Articles intended for the Original Department of the GAZETTE will be accepted only with the understanding that they are contributed to it exclusively.

Business communications should be addressed to the Publishers.

Leading Articles.

PHENACETINE.

APPARENTLY one of the best of the modern antipyretics is a substance described by Hinsberg and Kast as para-acetphenetidin, a substance analogous as regards its chemical constitution to antifebrin. We have already a number of times alluded to the properties of this substance (THERAPEUTIC GAZETTE, 1888, pages 43, 142, 699), and although the testimony as to the action of this preparation as an antipyretic and antineuralgic appears to be unanimous as to its value and freedom from danger, it has attracted no attention among English-speaking members of the profession. This preparation, phenacetine, as first prepared, was a reddish, odorless powder, insoluble in water and in glycerine, and thoroughly soluble in hot alcohol and alkaline liquids. It has been recently prepared in colorless, crystalline needles, which are claimed to be soluble in acetic and lactic acids and in hot oils. Extended experiments on dogs and rabbits have shown that it is almost inert in doses of from 15 to 30 grains given daily for days at a time. When the dose is increased up to 45 and 75 grains in large dogs it produces accelerated respiration, sleepiness, disturbed co-ordination, and vomiting, and, in still larger doses, methæmoglobin is produced, as in antifebrin-poisoning. Even after this symptom has appeared, however, recovery has almost invariably occurred. Dr. Hoppe (*Deutsche Medizinal Zeitung*, 1888, No. 92) has made a number of experiments on man, administering doses of from 15 to 40 grains, and has found that after a time the system becomes accustomed to the remedy. The only disagreeable effects produced by these amounts were sleepiness, dizziness, nausea, and slight chilliness, the temperature, as consequent on doses of 30 grains, being only reduced a few tenths of a degree Centigrade. It appears, therefore, to be almost free from toxic properties, while his experiments made in the Jewish Hospital in Berlin have confirmed the result of the experiments as already published by other observers. It has proved absolutely harmless, the only disagreeable after-effect being profuse perspiration, ringing in the ears, followed by weakness, and only in individuals already depressed by disease. As an antipyretic in children, doses of about 2 grains reduce the temperature from 1° to 2° C., a single large dose producing more effect than repeated small doses.

Led by the analogy of phenacetine in composition to antipyrin and antifebrin, Dr. Hoppe has likewise tested the properties of this drug in twenty-five different forms of neuralgia, in the majority of which relief of pain followed its employment. Various cases of headache were also relieved by its use within half an hour to an hour; and he believes that in neuralgia, as in febrile disease, it is equally as efficacious as antipyrin, and is preferable to it on account of its freedom from danger.

Dr. Rumpf (*Berl. Klin. Wochenschr.*, No. 23, 1888) likewise experimented to a considerable extent with phenacetine as an antipyretic and in the treatment of neuralgia. Dr. Rumpf believes that as an antipyretic it is as active as any yet introduced, since he has found that a single dose of 15 grains given to adults may reduce the temperature in the febrile state from two to three degrees in two hours; even half the dose has produced the same effect with no disagreeable complications. In eight cases of hemicrania doses of 15 grains produced great relief, while in seven cases of neuralgia of different nerves it has likewise been very satisfactory. Dr. Rumpf describes phenacetine as a drug which, in doses of 15 grains to adults or 3 grains to

children, is an absolutely safe, reliable, and satisfactory antipyretic; while in doses of 15 grains it is highly recommended as an anti-neuralgic remedy in all cases of vaso-motor neuroses, in the lancinating pains of tabes and the neuralgias of chronic neuritis.

Drs. Misrachi and Rifat (*Bulletin Gén. de Thérapeutique*, June 22, 1888) have confirmed in all respects the statements which we have already published as to the action of this remedy. It is these authors who have determined the solubility of phenacetine in lactic acid. They have found that this solution is not disturbed by the addition of water as long as the temperature does not fall below 33° C. It is evident that this discovery is a great addition to the practical value of this remedy, since its high insolubility has been the principal objection to its use in therapeutics. After the administration of the lactic-acid solution it is rapidly absorbed, and has been capable of detection in the urine.

COCAINE POISONING.

THERE have been recently reported several cases of cocaine poisoning which seem to be worthy of note. In a case of chronic cystitis under the care of Dr. Alejandro Settier, of Madrid (*La Medicina Practica*, December 15, 1888), the patient had been in the habit of injecting daily into the bladder twenty to thirty grammes of a four per cent. solution of cocaine. Dr. Settier, wishing to use injection of nitrate of silver, in order to render the bladder insensitive, injected a four per cent. solution of cocaine. On the second day of this treatment the solution of cocaine was allowed to remain for twenty minutes, and afterwards, the caustic solution having caused much pain, a second cocaine injection was given, and retained fifteen or twenty minutes. A few minutes after this the patient began to talk with the greatest volubility and a thick quavering voice with indistinct utterance. He complained of giddiness, was restless, with an unsteady gait, dry whitish tongue and mouth, cold perspiration over the whole body, and small, thready, rapid pulse, and several times repeated vomiting.

Much more serious cases have been reported by Dr. Zambianchi, of Vigevano, and Dr. A. Montalti, of Florence, (*British Medical Journal*, February 16, 1889.) In Zambianchi's case about two hundred and twenty-five milligrammes of cocaine were injected hypodermically into the breast. Almost immediately epileptiform convulsions came on and lasted fifteen minutes, with arrest of respiration. Under artificial respiration the woman rallied momentarily, but the convulsions reappeared, and five minutes later death occurred. In the case of Montalti 1½ grammes of the hydrochlorate of cocaine were given by the mouth, through mistake, to a patient suffering from phthisis. Fifteen minutes afterwards wandering delirium appeared, with ineffectual vomiting, marked fall of temperature, pale face, dilated pupils; cyanosis soon became apparent, and the pulse grew imperceptible. Unconsciousness developed, followed in a very short time by death. At the post-mortem, intense congestion of the brain and spinal cord with their membranes was found; the brain-surface being covered with a thin layer of blood and the subarachnoid space full of serum. On section of the brain-substance minute bleeding points everywhere appeared, giving rise to confluent drops which spread over the whole cut surface. The heart was firmly contracted; the spleen, liver, stomach, and small intestine were excessively congested; the kidneys normal. Another fatal case of cocaine poisoning was recently the subject of inquest at the University College Hospital, London. A 20-grain solution in a glass of water had been ordered by the house surgeon without direction, but with the word "statim" written upon it, a customary indication that the solution is to be dispensed immediately; the apothecary, misunderstanding this, gave twenty grains of cocaine in solution in a measure-glass to the nurse as a draught for the patient. The initial symptoms are not known, for it was not until nearly an hour had elapsed that the nurse, hearing the patient making some unusual noise, had her attention drawn to him. At this time there was coma, out of which the patient could be partially aroused with convulsive movements of the limbs. At the autopsy the brain was found intensely congested, with serous effusion beneath the arachnoid. It was also noted that there was extensive tubercular disease of the kidney and bladder, as well as in the lungs.

HEMORRHAGE FROM SALICYLIC ACID.

AS long ago as 1886 and 1887, Dr. Lauriston E. Shaw, Medical Register to Guy's Hospital, recorded cases to show the influence of the salicylate of sodium in the production of hemorrhage during the treatment

of acute rheumatism. These cases were one of renal hemorrhage and two of hæmaturia. In the London *Lancet* for January 19, 1889, he further quotes two instances in which very distinct hemorrhage occurred from the gums in rheumatic patients treated by the salicylates. We do not think in America that it is generally recognized that the salicylic acid and its preparations have a tendency to produce hemorrhages; but Dr. Shaw affirms that the occurrence of epistaxis during salicylation is mentioned by nearly all writers on the subject, and was freely accepted at the discussion by the London Clinical Society in 1881. Of one hundred and seventy-four patients treated in Guy's Hospital by these drugs, the records show that six per cent. were recorded to have had hemorrhages. The author further affirms that, after having seen so many cases, he has never seen epistaxis occur until the deafness, headache, vomiting, tinnitus aurium, and irregular slow pulse have been well pronounced as the result of very large doses. We think in this country that it is very rare for the salicylates to be exhibited to the extent of causing irregularity of the pulse. Certainly we never saw such a thing. Possibly this is the reason why hemorrhage has never occurred under our observation, and we would very much like if some of our readers would give information as to its occurrence in their experience. In judging of the frequency of such a symptom from published reports, it must be remembered that the man who has seen it and who believes in its existence writes, but that he who has never seen it, and possibly believes that there is no possible relation between the recorded hemorrhage and the drug alleged to be the cause of it, is almost sure to not put his views forth on paper. The negative rather than the positive side of any such therapeutic or pathological study is always much more positively represented in journals.

THE NARCOTIC PROPERTIES OF ESCHSCHOLTZIA CALIFORNICA.

IN our last issue we referred to the claim advanced by Messrs Bardet and Adrian as to the narcotic properties of the *Eschscholtzia Californica*. These authors, it will be remembered, claim that they have succeeded in extracting a glucoside, an organic base, and a small quantity of morphine, from this plant. In the *Gazette Médicale de Paris* for February 2, 1889, is an account of some experiments made by Dr. Ter-Zakariant, in the service of Dujardin-Beaumetz, with a watery and an alcoholic extract of this plant. The alcoholic extract is soluble in alcohol, with the exception of the resin soluble in water, but very slightly soluble in glycerin, and insoluble in alcohol and ether. It has a strong, agreeable odor, and a very bitter taste. From an attempt to determine the physiological action of this extract, the author has found that a few minutes after the administration of this substance the respiratory movements increase, and are then gradually reduced, until death takes place. In small doses it has no marked action on the temperature, while in large doses it increases or decreases the temperature, according as it is administered, pure or free from resin. Finally, in small doses, its action on the nervous system appears to be confined to the arrest of the cerebral functions. The animal loses all power, the voluntary movements are entirely suppressed, and the animals are entirely indifferent to all external stimuli. The functions of the spinal cord and the medulla are only affected by large doses,—the motor nerves are first affected, and the sensory nerves only some time afterwards. It is evident that this very incomplete account of the action of this plant adds but little to our knowledge of the properties possessed by this drug, further than the fact that it evidently contains some principle which is worthy of isolation. The experiments made to determine its therapeutic properties appear to show that it is a narcotic of considerable activity and apparently harmless. The dose employed of the alcoholic or watery extract is from thirty to one hundred and fifty grains a day. We trust that further experiments will be made to determine, in the first place, whether this plant does actually contain morphine; and, second, if that be not the case, as to what ingredient its activity is due; while it is evident that if it possesses any value whatever, it must be subjected to much more study before its properties may be applied in therapeutics.

IODIDE OF POTASSIUM IN SYPHILIS.

WE feel somewhat inclined to apologize to our readers for occupying so much space as is given in this number to the subject of iodide of potassium; and, when we agreed to receive the letters, had no idea that they would be so long. The subject is, however, a very important one, and the writers of

the letters are men of experience and renown. So possibly we have not overleaped the boundaries, and, in any case, the discussion is now closed.

Reports on Therapeutic Progress.

PRIMARY UNION AFTER EXCISION OF TUBERCULAR HIP-JOINTS.

At the meeting of the Royal Medical and Chirurgical Society, held December 11, 1888, MR. ARTHUR E. BARKER read a paper with the above title, emphasizing certain conclusions put forward by him in his Hunterian Lectures at the Royal College of Surgeons last summer (*British Medical Journal*, December 15, 1888).

Briefly stated, these were as follows: 1. That scrofulous or, in other words, tubercular joint-disease was a local expression of infection of the tissues with the bacillus tuberculosis, and ran, as a rule, a course beginning in hyperplasia and ending in fatty degeneration. 2. That at a certain stage of the disease the *complete* removal of the infected tissues ought to be possible. 3. That if complete removal of the tubercular tissue from the hip were accomplished, the resulting wound ought to heal in many instances by first intention throughout, as in the case of the knee, and often even without any necessity for drainage. The great advantages which would flow from such primary union after excision were then briefly discussed. The author then described his first case operated upon after the above propositions were made last June, and showed that they were justified by the result. The patient was a boy, aged 5 years, who had been under careful treatment for a year for hip-disease, which resulted in destruction of the head of the femur, and the formation of an abscess of the usual kind. After operation on the lines indicated, primary union took place without drainage under one dressing, and the patient left hospital on the fourteenth day wearing a double Thomas's splint. The latter was removed in the eighth week after operation, and the child went to the seaside, when he began at once to run about, and has been quite well ever since. Other cases were alluded to in which Mr. Pollard had since obtained similar results as regarded primary union by operating on the same principle.

MR. BILTON POLLARD, in reading his paper, drew attention to the fact that the wounds necessary for the extirpation of tubercle from joints would heal as rapidly and as permanently as other wounds of a similar size if certain essential conditions were secured: 1, the whole of the tubercular growth must be removed; 2, perfect asepsis must be assured; 3, bleeding must be checked, and the wound made as dry as possible; 4, oozing must be checked by the even elastic support of a wool dressing and a moderately tight bandage; 5, absolute rest of the part must be maintained during the process of healing. The hip joint was selected for illustration of the views advanced because the author thought that, of all excised joints, the hip had been found the slowest to heal. Four cases of advanced hip-joint disease with caseous abscesses were referred to. Three were operated upon by the anterior incision, and the fourth by a curved incision round the upper and posterior part of the trochanter. Drainage-tubes were not employed in any of the cases. The dressings were changed on the seventh day after the operations, and the wounds were found to have healed by first intention throughout. With the exception of one case in which the acetabulum was perforated, there had been no recurrence of the disease. It was hoped that three of these cases might be brought to the meeting for inspection by the Fellows of the Society.

MR. HOWARD MARSH had been glad to hear that Mr. Barker's case was in the anteroom, and requested that if possible it might be shown to the Society. The little boy then walked in with a considerable limp and some adduction of the leg which had been operated upon.

MR. BARKER explained that he had had a fall lately, and had bruised his knee, which accounted in part for the limp; and also that his splint had been discontinued too early when he was no longer under his own observation; and further, that he was not wearing any high-heeled boot.

MR. CROFT congratulated the authors of the papers most heartily. The success of this treatment might fairly be said to mark an epoch in the surgery of disease of the hip-joint. He could easily remember the condition of treatment seventeen or eighteen years ago, when it was what we should now think deplorable. No operations were then performed until the last stage, and the results were very disappointing to the operating surgeons. In 1880 he had brought a paper before the Clinical Society, treating of forty-five cases, in which he had excised the hip-

joint, and the mortality in those cases was thirty-five per cent. General opinion then was decidedly against early excision. A committee of the Clinical Society was appointed, and reported that it was undesirable to excise the joint unless there was dislocation or a sinus, unless, in fact, the disease was in the last stage. He had continued his practice of early excision, and since then had had fifty-three operations, with a death-rate of only fifteen per cent. That, at all events, was a great gain. When he saw the cases before them to-night he felt ashamed that there should have been so many as fifteen per cent. Of these fifty-three cases, one had been cured within a month, ten within six months, eighteen within a year, and twenty-seven within two years. So far he had been able to make himself certain, and that he considered a very great progress. He envied the authors of the papers, who were able to speak of curing their cases in weeks instead of months; that the wound should be healed in a week would have been an astounding point in practice not long ago. Eight years ago it was a question whether to excise at all. He congratulated English surgeons in recognizing that there was such a thing as tubercular disease which could be cured. He had not himself used the anterior incision, the deep sutures, and various other modern improvements.

Mr. Bryant did not think he could accept Mr. Croft's conclusion that there must be operation in hip-disease at an early stage. More evidence was wanted before that could be decided. The cases that had been shown them were excellent evidence as far as they went, but, of course, the end of these cases had not come yet. He should have been glad to have seen the joints that had been removed, for they must not forget that joints may be repaired; they might have healed without an operation. He was quite ready to admit that there actually was mischief that nature could not heal; in the case in which a sequestrum of bone had been extracted there was certainly need for an operation. He wished to leave out of account all question of whether there was tubercle or not, and then it was the degree of disorganization that determined the operation. He was still in favor of expectant treatment in the great bulk of cases. Of course, if early excisions were compared with an equal number of late excisions, it would be *a priori* probable that there would be more recoveries among the early cases. He would remind Mr. Croft that the cases collected by the committee of the Clinical Society in 1880, which gave a death-rate of thirty-five per cent., were taken from all the hospitals in London, and at a time when the whole knowledge of the treatment of wounds was very different. He could hardly think Mr. Barker would have wished that his patient should have left off using a splint so soon as eight weeks after the operation. As they had seen, a considerable deformity had resulted from it. It would have been better that he should keep it on for a year. Mr. Pollard had his cases on splints, and he had no doubt would keep them there.

Mr. Hulke felt he ought to begin by congratulating Mr. Barker and Mr. Pollard on the results of their treatment. Still, many cases, if treated with rest in the early stage, would recover without excision, and when they had recovered in that way gained stronger limbs, were more fit for work than cases of excision. When cases of hip-joint disease were spoken of as healing in a few weeks, he supposed the external wound was all that could be referred to; there would be no complete bony union. In his own cases the mortality had never stood as high as at thirty per cent., and did not now reach fifteen per cent. He had never been in the habit of cutting off the great trochanter, as he was surprised to learn had been the custom with some operators. He was pleased to hear that Mr. Pollard and Mr. Barker were able to get out the synovial membrane through a post-anterior incision, for he should have thought it quite impossible, considering its many folds and recesses. They had heard at a recent meeting how difficult it had been in arthrectomy of the knee when the joint had been laid freely open, and he should have expected it to be much more difficult in the case of the hip. The operation by anterior incision was no new thing; it was introduced by Hüter, who combined it with a drainage-tube at the back of the joint.

Mr. Howard Marsh had never seen such good results as the cases they had before them. If they turned back eight or ten years, the operations on the hip-joint were certainly of a much rougher type than at present. His usual method had been by posterior incision, but he thought from these cases that an anterior incision would be better. The operations were not quite new; they had all seen a tubercular joint heal quickly after incision. He had treated a knee by excision, and left the wound untouched for a week, and found it then completely healed. Mr. Barker's case was to him disappointing, for the deformity

was considerable. The most important question of all was not the best way of obtaining primary union, but the ultimate conditions of the limb; and the difficult point to decide was what condition could be reached by long rest at first and good nutrition, as compared with early excision, and it must be remembered that some evidence was accumulating against the permanence of tubercular disease.

Mr. Croft begged to be allowed to remark, *à propos* of what Mr. Hulke had said, that when he admitted a mortality of fifteen per cent. in fifty-two patients on whom he had operated, he did not mean that these deaths had been in consequence of the operation; with one exception, in fact, they had been all due to tubercular disease at some time subsequent, and not consequent on the operation.

Mr. C. B. Keetley remarked that after he had excised a diseased hip-joint and scraped out the morbid surrounding tissues, the case had done very well for six weeks, but after that, had died suddenly from tubercular meningitis, and he had thus had an opportunity of examining the joint, and found the head of the femur firmly joined to the acetabulum, and hardly any trace of cicatricial tissue left. The use of the drainage-tube had not been thoroughly discussed, it had been treated as a thing to be avoided, if possible. He regarded it himself as a safeguard against the mistakes in antiseptic treatment into which it was always conceivable that they might fall. If all the morbid tissues had not been removed, there would be use for the drainage-tube in lessening the chance of self-infection. It did not in the least prevent early convalescence; cases might be up and going about in a fortnight on crutches without being the least the worse for a drainage-tube.

Mr. Hulke would be obliged if Mr. Barker in his reply would tell them in more detail how he had found it profitable to fill such a large cavity as was left after excision of the hip with an emulsion of such a strong poison as iodoform.

Mr. G. A. Wright, on being invited by the President to speak, said that he had had considerable opportunities for watching the results of hip-disease among the tuberculous population of Manchester. Out of the last eight hundred cases he had seen, he had performed excision in one hundred and thirty-five. He agreed that the best results would be obtained from rest if the cases were taken in time; but that qualifying clause made the maxim unpractical. He had never seen better cases than what had been shown him that evening. He used drainage-tubes himself, and did not get the operation wounds healed so quickly. In these the section of the bone had been above the trochanter; he thought that no better than section through it. The great difficulty was the entire removal of the morbid tissues. He had found it practically impossible either by the anterior incision, which he had used occasionally, or by the posterior. Only little foci of disease might be left, but they were apt to suppurate when some fall or accident to the child gave them an opportunity. The cases Mr. Barker and Mr. Pollard had shown could hardly be called early cases. The difficult point to decide was whether, in cases of this sort, the children could stand the long waste of getting well without the surgeon's knife. He was accustomed, even after excision, to keep on a Thomas's splint for a year, for without it they were liable to many dangers.

Mr. G. H. Makins related his experience in Halle in 1879, when Volkmann was performing many excisions in early cases of tubercular hip-joint disease. He removed the synovial membrane most carefully, and used a weight after the operation, but no splint. Many of the cases hopped about the wards in three weeks, supporting themselves on a little truck in front of them, and at first seemed to be healing very well. In 1887, however, the practice was given up, for the ultimate results had been unsatisfactory.

Mr. Barker thanked the Society for the kind way in which they had received his paper, but could not claim competence to answer all the questions that had been raised. In the matter before them they were all to some extent in the position of learners. The first question was, What was early hip-disease? He was in the same position as every one else in not wishing to excise the joint until he had given up hope of curing it otherwise. This test case that he had brought forward had had rest to begin with for a year. Before the operation abscesses near the joint had been aspirated, and when the joint was finally laid open it was in advanced disease, and the head of the bone carious throughout. Still he thought the child was better for the operation. He was in favor of using a Thomas splint for months after the operation, as long, in fact, as he could. In this boy's case it had been a mistake of his parents to leave it off;

still it made the case, perhaps, more interesting, and he hoped to get his leg right again, but it certainly was not the treatment he advocated. In answer to Mr. Hulke, he remarked that the cavity left after an operation by anterior incision was a very small one, and he agreed that the iodoform emulsion should be used in small quantity. In order to avoid strong germicides in children he had used an abundance of hot water, and only a little iodoform.

MR. BILTON POLLARD felt he had to return thanks to all who had spoken, and especially to Mr. Wright, who had expressed what he had to say himself. He considered the cases he had shown as rather of late than early disease, since there had been carious bone and abscesses just going to break through the skin. He agreed with Mr. Barker that there was very little cavity left after excision by this plan, for it was filled up by the soft tissues.

A NEW ANTIDOTE FOR MORPHINE.

In the *Internationale Klinische Rundschau* for January 27, 1889, PROFESSOR ARPAD BOKAI recommends picrotoxine as an antidote for morphine, on the ground that it exerts an antagonistic action to morphine on the respiratory centres; for, while morphine tends to paralyze these centres, picrotoxine exerts a powerful stimulating effect. Since, therefore, death in morphine poisoning is usually attributable to paralysis of the respiratory centre, on this ground alone picrotoxine should be indicated as a valuable antidote. Further, morphine may produce such rapid reduction in blood-pressure as to endanger life; while picrotoxine, on the other hand, is a powerful stimulant to the vaso-motor centre, and is in this respect also an antagonistic to morphine. Professor Bokai adds that the action of morphine on the cerebrum is directly opposed to that exerted by picrotoxine. Finally, Professor Bokai suggests that the previous administration of a small dose of picrotoxine might reduce the danger of asphyxia in chloroform narcosis.

CITRATE OF CAFFEINE IN ECLAMPSIA.

MR. BOLTON CORNEY divides cases of puerperal eclampsia into those in which the affection is due to the retention in the blood of excretory matters through defective action of the kidneys, and those which appear to arise independently of such a cause. In the latter class of cases he maintains that caffeine is especially valuable, and he reports the following remarkable case, which certainly appears to substantiate his claims (*Practitioner*, February, 1889):

Mrs. S., a married lady, 23 years of age, gave birth to an undersized but full-time child at 7 A.M. on August 21, after an easy labor of only three hours' duration. The placenta, etc., came away as usual.

August 21.—There was some distress from after-pains, and about noon the patient began to complain of headache, which became worse. At 9 P.M. she vomited often, and at 11 P.M. she became violently convulsed, as in epilepsy, with total loss of consciousness. The fits continued, with short remissions and intermissions, for ten hours, during which half an ounce of potassium bromide, a quarter of a fluidounce of tincture of hyoscyamus, and forty grains of chloral hydrate were administered by the practitioner in attendance. A few whiffs of chloroform vapor were also given.

There was no œdema of face or feet, the urine contained no albumen, and there had been no signs whatever of kidney-disease or of temporary derangement of the renal function. The patient remained in a deep stupor for three nights and the two intervening days. She continued to swallow readily all food, drink, or medicine which was placed upon her tongue. The bladder became paralyzed, and the urine dribbled all the time. The lochial discharge was very scanty and glutinous. The temperature maintained an even figure,—101.5° F., but the pulse showed remarkable fluctuations between 80 and 132 in the complete minute, and there were periods of cyanosis. Respiration was for the most part at the rate of 30 per minute, but occasionally more. Although the trachea was from the first very noticeably obstructed with mucus, the patient never once coughed, and therefore did not clear it. It was not of such amount as to cause cyanosis or any embarrassment in breathing. Some other reflexes were inactive, but the act of swallowing remained perfect throughout, although consciousness was entirely absent. Beef-tea, milk, white of egg, and now and then a teaspoonful of brandy, were given at brief intervals.

Matters became worse, and the continued and complete stupor became hourly more alarming. In the afternoon of the 23d the case appeared to the patient's relatives and friends hopeless. The left arm and leg were

paralyzed, and the right fingers and hand were picking at the bed-clothes. There was partial unilateral ptosis, and drooping of the corner of the mouth, causing a blowing respiration through the gap. Teeth and tongue were dry and covered with black sordes. Pupils were insensible, equal, conjunctivæ and corneæ glazed with a death-like, glairy mucus; the gaze fixed; the beads of clammy perspiration were on the forehead, lips, and wrists. Bowels refused to act, even after turpentine enemata. Urine dribbled all the time, and was a source of much discomfort and danger. Pulse, 110; breathing, 32 to 36; temperature, still at 101.5° F. The heart fluttered; lividity occurred more often and lasted longer. At this stage of the case three grains of citrate of caffeine, with two and a half grains of sodium salicylate in distilled water were injected subcutaneously. This was followed in an hour by 6 grains more given by the mouth, and 2 grains every two hours afterwards for six doses. Some general improvement took place, the pulse improved, and the attacks of lividity ceased. The paralytic symptoms diminished in degree, and towards dawn on the 24th signs of returning consciousness were observed.

August 24.—At 10 A.M. the pulse had fallen to 82, and was firm and full, and respiration was at the rate of 26. The pupils had regained some sensibility, but the temperature remained the same,—101.5°. The lochia, however, were becoming very deficient, and warm vaginal douches of carbolized water were given every hour. The genitalia were sponged with a stronger lotion, and the whole body was washed with the same.

The caffeine was suspended.

At 3 P.M. there was another attack of imperfect aëration, with lividity; the patient seemed to be losing ground again, the respiration had risen to 32, and the (thready) pulse to 110. Resuming the caffeine, 2½ grains every two hours.

August 25.—1 A.M., consciousness returning. Eyes open and clear. General improvement. Skin hot and dry, however. 6 A.M., is undoubtedly awake, though her mind seems a void. Answers "yes" and "no," but with some hesitation. Lochia ceased, but nurse omitted to report it. 5 P.M., skin very dry and hot, face flushed. Frequently places hand over pubes, as if in pain there. Breathing, 36. Trachea now free from mucus; patient able to cough. Pulse, 110; temperature, 103.8° F. Nurse now admits that lochia ceased this morning. Hot fomentations. Quinine, 15 grains, statim.

August 25 to 26, midnight.—Now much more awake. Speaks whole sentences, but is confused, and has entirely lost memory. Picks at buttons or anything bright within reach. Very hot and flushed. Temperature, 104.2° F.; pulse, 130. Repeat quinine, 15 grains. Continue fomentations and sponging. Discontinue caffeine.

August 26.—The lochial discharge returned in the morning, and was of more normal character than it had ever been. The symptoms relating to it were all ameliorated, the temperature fell in a few hours to 98.1° and the pulse to 88. From this time the patient made a steady recovery, and a week later the only remaining abnormities were a certain degree of muscular weakness and debility, with the usual singular blankness of memory for recent events, including not only the labor and its concomitants, but the occurrences of the previous two or three months.

A HITHERTO UNDESCRIBED CAUSE OF DELAY IN THE SECOND STAGE OF LABOR.

At the recent meeting of the Buffalo Obstetrical Society, Dr. A. H. BRIGGS read a paper with the above title. (*Buffalo Medical and Surgical Journal*, February, 1889.) This subject was suggested to him by observations at the bedside. In the divisions of the stages of labor, he gave the second stage as the period from complete dilatation of the os to final delivery. In anatomical studies of the uterus, it had been observed that its circular structure was composed of two kinds of fibres, viz.: circular and longitudinal. The longitudinal fibres belonged to the upper portion of the womb, while in the cervix they were more circular, the two being somewhat interblended.

The phenomena of dilatation consisted in the drawing back of the circular fibres by the longitudinal ones, not alone from the expulsive pressure of the uterine contents, but from contraction of the muscular fibres of the womb, due to the muscular action named. Taking that for granted, one could easily see why dilatation occurs without much pressure of the head. In some cases as the edges grow thinner, they grasp the head like a band or cord, and finally can be felt sliding over it. This produces an agonizing pain, just at the moment the os slips over the head; then there is repose, so far as active contractions of the

uterus are concerned, and the patient, too, is quiet.

The speaker called particular attention to the part the longitudinal fibres play at this time. He believed that in many cases of primiparæ, the os grasped the neck just at this period of labor, holding it tightly, which was one cause of delayed second stage.

When watching for the advance of labor at this time, he had been struck with the great delay, notwithstanding active pains, as well as other evidences of apparently progressive labor.

In relating a case where he had met substantially this difficulty, he had gone to fetch his forceps, but, on returning, he found the woman had delivered herself. He had not observed this phenomenon stated in the literature of the subject as a cause of delay in this stage of labor, but was convinced from his observations that it was to be regarded as a factor in, or cause of, delay many times.

THE TREATMENT OF SUFFOCATIVE ATTACKS AFTER TRACHEOTOMY FOR CROUP.

Pieniazek has sought for some years to diminish the large mortality after tracheotomy, and used for some time, and with considerable success, an elastic catheter followed by aspiration. The catheter seemed to owe part of its success to its irritation of the tracheal walls, whereby a violent and effectual cough was set up. The method succeeded for the first or second severe attack, but, after that, as a rule, the stenosis reached the bronchi and it of course failed. In September, 1882, he first tried another method upon a female child, aged 2 years. The patient being so young, and the trachea consequently so small, but little benefit was likely to be derived from the catheter treatment; moreover, its insertion might cause complete obstruction and suffocation. The author, after tracheotomy had been performed, remained within call, and when the first fit of suffocation (which was very severe) occurred, he removed the canula and first attempted to aspirate through the catheter; but, as this did not succeed and the child's condition began to be critical, he introduced a pair of Schrötter's laryngeal forceps, with the blades placed antero-posteriorly so as to avoid catching hold of the trachea at its bifurcation, and succeeded in bringing out a knuckle of pseudo-membrane. Artificial respiration followed, and the patient soon breathed freely. During the next three days four other attacks were similarly and successfully treated, the membrane being first pushed down with the catheter and afterwards seized and removed with the forceps. One of these membranes was a complete cast of the lower part of the trachea with the three principal branches of the right and left bronchi.

The treatment of the sixth attack, however, failed. The removal of the membrane was followed by free vesicular breathing all over the right lung, but over the left nothing was audible. Two hours later the child died quietly, with symptoms not of stenosis, but of collapse.

The next case was that of a boy, aged 7, in whom, twelve hours after tracheotomy, a violent suffocative attack occurred. The catheter and forceps were, however, successfully used. After that about four attacks took place daily, but these were treated before the symptoms became so acute, and often without the preliminary aspiration, as this was, as a rule, inadequate to remove the membrane. The attacks continued, but with diminished frequency, up to the third week, and the later ones were treated at once with the forceps; this instrument, instead of the catheter, being used to force down the membranes. Later still, it was found that the trachea was free, and that the obstruction was exclusively in the bronchi. The author then lightly curved the forceps, and by sense of touch, together with observation of the length and the direction of the curve of the instrument, succeeded in passing it into one or other bronchus. At the end of three weeks the attacks ceased, and at the end of five the child was removed.

In another case—that of a child, 2 years old—there was a considerable degree of stenosis after the operation, and its seat was made out to be under the canula. The author was anxious to see it, in order to determine its nature, and for this purpose, after several failures, he took a large ear-speculum, pushed it into the wound, bent the patient's head as far back as possible, and pressed the head of the speculum towards the spine, so that the smaller end pointed to the bifurcation of the trachea. On throwing light from a reflector, the stenosed portion was plainly visible, and was seen to be due to growths upon the hinder and lateral walls; after failing to remove them blindly, a pair of laryngeal-polypus forceps was introduced through the speculum and two of them were pinched off, the remainder being treated with the gal-

vanic cautery. The operation was not easy, as the instrument shut out nearly all the view, but the difficulty of determining how deep to go was got over by measuring the distance first with a sound and marking it off on the forceps. The operation had, however, to be repeated more than once.

To reach the false membrane and dry masses in the bronchi and their principal divisions, Dr. Pieniazek has had an instrument like a very long blunt ear-spoon made. This was first used in a boy, aged 5 years, and though death appeared imminent he recovered. The spoon is of especial advantage in the case of the secondary bronchi, the walls of which are so thin that it is difficult to avoid seizing them with the forceps. The symptoms of stenosis in this situation were such as to be readily confounded with those of adynamia, for the breathing was superficial and there was no stridor; auscultation, however, always gave localized absence of vesicular murmur. During the year 1887 the author lost seven out of twenty tracheotomy cases,—namely, two of erysipelas, two of adynamia, one of pneumonia, one of bronchostenosis (which it was found impossible to relieve), and one was already dead in a suffocative attack when reached. In conclusion, the author enjoins that these methods should not be resorted to unless either danger is imminent or milder measures have failed.—*The London Medical Recorder*, January 21, 1889.

THE THERAPEUTIC ACTION OF HYOSCINE.

In the *Therapeutische Monatshefte* for January, 1889, Dr. Kny, of Strasburg, reports the results of the administration of the muriate of hyoscine in subcutaneous injections in eighty-eight different cases occurring in the Strasburg clinic for nervous disease. In 82.2 per cent. the result was successful, sleep lasting from six to eight hours occurring within an hour after the administration of the dose. The majority of failures occurred in cases where the insomnia was not accompanied by any motor disturbances; while, on the other hand, where there were marked motor symptoms, as in mania and paralysis, the result was the most favorable. The dose was from $\frac{1}{125}$ to $\frac{1}{64}$ gr., although sometimes the dose had to be increased on account of the patient becoming accustomed to the drug. One-twentieth of a grain a day was the largest amount ever given. Disagreeable after-effects were seldom observed, and these consisted of dryness of the throat and thirst. Heart-disease does not appear to be a contra-indication for its use, since it was used in a case of aortic insufficiency with good result. Tastelessness, ready solubility, and cheapness are special recommendations for the drug. Dr. Kny gives the preference to hyoscine for a hypnotic in case of great excitement; while sulphonal is preferable in cases of insomnia not so accompanied by motor disturbances. In paralysis agitans and multiple sclerosis hyoscine proved to be a palliative remedy.

THE TREATMENT OF CARDIAC DILATATION.

At the eighty-third annual meeting of the Medical Society of the State of New York, recently held in Albany, Dr. A. L. Loomis, of New York, read a paper on the Management of Cardiac Dilatation based on its Etiology, of which the following is an abstract. (*Medical Record*, February 9, 1889.) Cardiac murmurs were now regarded as of significance according to the pathological lesions and functional disturbances which they attended.

When the circulatory system became disturbed by disease within, or aside from, the heart, there was tendency to compensating hypertrophy. To maintain such hypertrophy of the heart certain conditions must be complied with: (1) the muscular tissue of the heart must be healthy; (2) the heart-muscle must receive sufficient supply of healthy arterial blood; (3) waste products must be rapidly removed from the body; (4) the nerves of the heart must be in normal state. Any changes of these conditions led to weakening of the heart-muscle and favored dilatation.

In studying the etiology of cardiac dilatation, cases divided themselves naturally into two classes,—primary and secondary. The primary causes were (1) impaired nutrition with extreme anæmia, as in those subject to excessive physical labor; (2) degenerative changes in the heart-muscle; (3) excessive use of narcotics, as tobacco, alcohol, excessive venery. Secondary causes were insufficiency of aortic and mitral valves, obstruction to aortic circulation, atheromatous changes in nutritive artery of the heart, arterial sclerosis, and renal disease. It was always important, from a therapeutic stand-point, to determine whether dilatation was or was not secondary to hypertrophy, for the chances of temporary or permanent relief were greatly diminished if it were secondary to hypertrophy. Dilatation from impaired nutrition,

most common in hard-working men, should be treated by rest, diet (mostly animal food), moderate physical exercise. He had obtained benefit from only one drug, arsenic in $\frac{1}{60}$-grain doses after meals. In dilatation after typhoid fever, and other acute infectious diseases, the patient should not be allowed to rise from bed; give suitable food, wine; give combination of quinine and strychnine. Attention to functions of stomach and liver. Small doses of calomel, combined with rhubarb and soda, diet, and rest, often did more to relieve symptoms than tonics. Iron, arsenic, and strychnine were the most useful tonics in cases of dilatation from fatty degeneration.

The general management of dilatation due to fibroid degenerations of the heart-muscle was similar to that in fatty degeneration. Bichloride of mercury was administered in $\frac{1}{16}$-grain doses for a long period; so-called cardiac tonics to be avoided; maintain action of the kidneys. Dilatation in those addicted to excesses with tobacco, venery, etc.; remove the exciting cause; use of strychnine and shower-bath; bromide of potassium in cases of excessive venery.

In secondary dilatation the condition was more serious. The heart-muscle should be kept as healthy as possible by diet and general habit; when compensation was incomplete digitalis was demanded. Digitalis was safely administered when pulse was of high tension, and its use was followed by increased urinary secretion. Some of the indications for convallaria, strophanthus, etc., were pointed out.

A NEW OPERATIVE TREATMENT IN EMPYEMA.

The treatment of empyema offers great difficulties when the lung is completely collapsed as the result of long compression by a purulent effusion. In such cases since 1879, Estlander and others have advocated the resection of large portions of the ribs with opening of the thoracic cavity. Schede, modifying this operation, removes not only the rib, but also its periosteum and subjacent pleura,—that is to say, he takes away the inner chest-wall, with the exception of the skin. Sprengel has introduced still another modification by making a subperiosteal resection of two ribs throughout almost their entire length, and then opening the pleura, and washing out the pleural cavity. This procedure, according to its introducer, has given excellent results where other means have proven unsuccessful. PROFESSOR SAUBBOTINE (*Gazette Hebdomadaire des Sciences Médicales*, January 26, 1889) thinks that the methods of Estlander and Schede are accompanied by great dangers from the fact that they leave a large denuded surface constantly bathed in pus, and may readily so lead to blood-poisoning. He thus has endeavored to preserve the surface of the wound from this contact. He starts with the idea that, to cure an empyema with complete collapse of the lung, it is unnecessary to remove the ribs, but that it is quite sufficient to perform on them a subperiosteal section. A rib being incised in two points, it is only necessary to exert pressure on the middle segment to reverse entirely the normal convexity of the rib, thus diminishing the cavity of the pleural sac. The following is his operative measure: The patient being anæsthetized, the skin is divided horizontally along the seventh rib, which is freed from its periosteum for a space from two to four inches in length. The pleura is then opened and the purulent contents evacuated and washed out with some antiseptic liquid. After that the wound is closed and sealed. An incision is then made along the border of the great pectoral muscle, and through this incision the fourth, fifth, and sixth ribs are divided, and a small place resected so as to make them pliable. A similar incision is then made in the subaxillary line and the same ribs are again divided. The two vertical incisions are then closed by sutures and then covered with antiseptic dressing. If the antiseptic precautions have been carefully observed, the wounds made in resecting the ribs heal by first intention.

The advantages of this operation over that of Estlander's are the following:

First, the costal wounds, if the necessary precautions have been taken, do not come in contact with the pus, and heal like simple fractures or subcutaneous osteotomies.

Second, the divided ribs becoming flexed greatly reduce the volume of the pleural cavity, and, consolidating later in this position, they furnish a sufficient support to the vertebral column, thus diminishing the tendency to deformity of the latter.

A HITHERTO UNOBSERVED EFFECT OF THE SALICYLATES.

Among the effects produced by the members of the salicyl group upon the nervous system, mydriasis has been noticed in association with the amblyopia frequently caused by the action of these drugs; but, so far as

we know, myosis has not been observed as a result of their administration. In a case reported in the *Practitioner* for January, 1889, by Drs. G. A. Gibson and R. W. Felkin, extreme contraction of the pupil was caused by the use of large doses of salicylate of sodium. The patient in this case was a lady of middle age, who fell ill on the evening of November 9. When she was first seen, early next morning, she complained of severe pains in the head and limbs, and the temperature was found to be 105.8° F., while the pulse was 104. In spite of the want of correspondence between the temperature and the pulse, it seemed probable that the condition was rheumatic in its nature, and the patient was ordered 20 grains of sodium salicylate every two hours. In the afternoon of the same day the pupils were found to be greatly contracted, and their reaction to light was absolutely lost, while the vision was distinctly impaired. Tinnitus aurium and deafness were also present, and were accompanied by severe headache, chiefly experienced over the occipital and parietal regions. The subsequent history of the case is of no importance in this connection. All that is necessary to mention is, that on the following day phlegmonous erysipelas made its appearance on the left leg, in consequence of which, along with the fact that the temperature had fallen to 101° F., the further administration of the sodium salicylate was suspended, and sulpho-carbolate of sodium was substituted for it at a later period of the day. On the following morning, the pupils, although still somewhat small, reacted to light, and it was found at the evening visit that they had returned to their usual size. About mid-day on November 13 the temperature again began to rise; a dose of salicylate of sodium was given, and repeated two hours later. At the evening visit, about nine o'clock, the pupils were found to be once more contracted, and to have lost their normal reaction. The effect had disappeared before the next morning. The patient made an excellent recovery. The myosis and loss of reaction to light appeared within eight hours of the commencement of the employment of the salicylate of sodium, and did not entirely pass away until thirty hours had elapsed from the time of its last administration.

RADICAL CURE OF HERNIA.

At a recent meeting of the Buffalo Medical and Surgical Association, Dr. Park read a paper on the "Radical Cure of Hernia." (*Buffalo Medical and Surgical Journal*, February, 1889.) After rehearsing the numerous methods of treatment that have been proposed, he took up the modern surgical procedure which has been advocated by various operators. He favored Czerny's method of ligating the sac as high up as possible and excecting the part below the ligature in all cases except congenital herniæ. In these the lower portion is left and closed by suture or ligature, so as to form a tunica vaginalis for the testicle, the part between the ligature being removed. Beginning from below, the pillars of the ring are brought together with wire sutures, leaving a small opening at the bottom for the cord. The author at first made a shoe-lace suture, but found it difficult to get accurate approximation of the pillars of the ring. Catgut was at first used for suturing, but was discarded later for silk. At present he employs silver-wire sutures, a sinus having formed in one of his cases with the throwing off of one of the silk sutures. The incision should be long enough, great care being taken to separate the sac from the cord. Intestines are replaced into the abdomen, and the sac ligated as high up as possible. The silver sutures are twisted, the cut end being turned in and left in place. Femoral and umbilical herniæ are to be similarly treated, extirpation of the sac being essential. "In strangulated hernia, after relieving the strangulation, proceed in the same way. Do not allow the wearing of a truss after the operation." The author has operated in forty cases, uncomplicated by strangulation or otherwise, with universal success. The patients were of all ages, from five years to sixty. Six of the operations were for umbilical, and eight for femoral hernia. In one of the cases of umbilical hernia, a fat woman, in whom the tumor hung down to the thighs, there existed a number of tumors within the sac, together with ulcerations on the surface. Fearing the tumors to be malignant, he had at first declined to operate, but, the patient assuming all risks, he operated, removed the sac and tumors, closing the wound with three layers of sutures, including the (1) peritoneum, (2) the deep and (3) superficial tissues. Adherent pieces of omentum are to be loosened and appendices of the same are to be securely ligated and cut off, especially if they cannot be easily replaced into the abdomen. Dr. Park takes the position that hernia, with its attendant liability to strangulation, is more dangerous to life than the operation for its

cure, and, therefore, recommends it in all cases.

CONTRAINDICATIONS FOR THE USE OF ANTIPYRIN DURING THE MENSTRUAL PERIOD.

Cases of toxic accidents from the use of antipyrin have been frequently reported, but the conditions under which these results are produced have not been sufficiently studied. Without doubt, in some cases, the poisonous effect is to be attributed to the poor quality of the drug, but the condition of the patient also deserves consideration. In the *Revue Générale de Clinique et de Thérapeutique* for January 24, 1889, DR. H. HUCHARD states that a year ago he administered 15 grains of antipyrin to a woman suffering from violent dysmenorrhœa. As the result of the administration of this drug, the menstrual flow was suddenly arrested. The patient was seized with violent chill, chattering of the teeth, the face became cyanosed, and there were frequent attacks of syncope, the pulse was small and weak, and the patient complained of great headache. The condition was such as to cause great anxiety for nearly an hour, when the effects gradually passed off. Dr. Huchard thinks that he has in two other cases observed similar symptoms, although less marked, and he now regards the presence of the catamenial flow as a positive contraindication to the use of antipyrin.

INTRAVASCULAR COAGULATION OF THE BLOOD AS THE RESULT OF DIFFERENT DRUGS.

Starting with the observation that certain substances, for example, lake-colored blood and blood of animals of different species, may produce, according to the amount of the injection, fatal thrombus or fatal hæmoglobinuria, DR. SILBERMANN (*Centralblatt für Klinische Medicin*, January 2, 1889) made a number of experiments as to whether certain substances which as yet are only known to produce hæmoglobinuria might not also produce coagulation of the blood while still in the vessels. It is evident that the determination of this question may perhaps explain the frequent occurrence of thrombus and the production of various morbid symptoms in different forms of poisoning. The author found that sodium chloride, glycerin, toluylendiamin, and pyrogallic acid produce thrombi; while animals in whom the blood of dogs poisoned with sodium chloride was injected, likewise died as the result of general thrombosis. Capillary thrombi were frequently found, and were recognized by the author by injecting indigo-carmine into the veins, and then seeing that certain parts of different organs were protected from the injection with this pigment. Dr. Silbermann also examined in this way the results of arsenic- and phosphorus-poisoning, and raises the question as to whether perhaps clinical symptoms with poisoning with these bodies may be attributable to coagulation of the blood in the smaller vessels by the production of thrombi.

A NEW SYMPTOM OF PERICARDITIS.

In the *Internationale Klinische Rundschau*, February 3, 1889, PROFESSOR VON STOFFELLA described a symptom occurring in pericarditis which appears to have been hitherto overlooked by all writers on this subject, with the exception of Dr. Pins, who has likewise made the same observation, and reported his entirely independent studies before the recent meeting of the Vienna Medical Society.

It is well known, that, on auscultation of the heart, in cases of pericarditis which are accompanied by a percussion-sound, the sound in different cases may either change its locality or its intensity. Thus, for example, a percussion-sound heard in any given point while the patient is in a recumbent position may either increase in intensity or entirely disappear when the patient sits up, and may appear in some entirely distinct place. The ground for change of character and locality of the sound is evidently to be found in the fact that in the change of position different parts of the pericardial surface come in contact. Dr. von Stoffella has found that in the change from the recumbent to the erect position in case of pericarditis there is not only an alteration in the sounds detected by auscultation, but also in the sounds recognized by percussion. He claims that any case where, in the recumbent position, effusion at the base of the heart may be recognized by percussion, when the patient assumes the sitting position the character of the percussion-sound changes completely. This phenomenon the author explains as follows: As is well known, pericardial effusion leads to the production of dulness by compressing the lung and so bringing a greater part of the pericardial surface in contact with the chest-walls. If, then, the pressure exerted by the exudation be reduced, it is evident that the lungs will again expand, and substitute

the resonance of the pulmonary tissue for the dulness of the pericardial effusion. In the recumbent position of the patient, accumulation of fluid in the pericardium exerts a much greater pressure on the lungs than when the patient is in the erect position, for in the latter case the effusion gravitates to the most dependent part of the paricardial sac, and to that extent relieves pressure on the lungs.

CHRONIC HEADACHE.

At the recent meeting of the Medical Society of the State of New York, DR. C. L. DANA, of New York, read a paper entitled "Chronic Headache" (*Medical Record*, February 9, 1889). It was based on a study of two hundred cases of chronic headache, functional in character, excluding migraine. His conclusions were: 1. There should be more general recognition and differentiation of chronic head-pains of functional origin. 2. Pains in the head are either neuralgias, migraines, or so-called headaches. 3. The seat of headaches is always in or referred to the trigeminal, vagus, or four upper cervical nerves. 4. Headaches are seated in or referred to the periphery of the nerves in question; neuralgia is seated in the trunks or central; migraine is a periodical discharging neurosis, affecting motor, vasomotor, secretory, as well as one or more cranial sensory nerves. 5. The dura mater is the most frequent seat of headache. 6. The great headache ages are from eight to twenty-five, and again from thirty-five to forty-five; childhood and old age are exempt. 7. The chronic functional, non-migrainous headaches may be classified as hæmic, including diathetic forms, toxic, dyspeptic, neurotic, reflex.

More exact knowledge of the nature of headaches may be gathered by studying the location and character of the pain in its relation to cause. Neurotic headache is best treated symptomatically by antipyrin, menthol, antifebrin, caffeine, and bromides. Headaches due to reflexes are often treated better constitutionally than by means directed to local conditions. Rheumatic headaches are best treated by ammonia, chloral, and warmth.

THE ANTILITHIC PROPERTIES OF ANTIPYRIN.

In the *Revue Générale de Clinique et de Thérapeutique* for January 24, 1889, H. HUCHARD writes that about a year ago he administered several doses daily of 15 grains of antipyrin for violent renal colic. The result in relieving pain was very striking, so much so that the patient contracted the habit, without advice, of taking two doses of 15 grains each of antipyrin every day. He continued this independent treatment for about six months, and then reported that his pain had entirely disappeared and that there had also been a great change in the renal secretion. The urine was entirely free from sediment, was clear, limpid, and chemical examination showed that it contained but a normal quantity of uric acid. This result was especially surprising, since it is generally acknowledged that antipyrin diminishes renal secretion. Dr. Huchard states that he hardly felt warranted in concluding that the improvement in this case was attributable to the antipyrin until he found a case reported by Schweig which had likewise been relieved not only of the renal colic, but that the renal secretion had been restored to its normal character by the daily administration of 1½ grains three times daily. These two observations are naturally not sufficient to establish the remedial value of antipyrin in the uric acid diathesis, though it seems to warrant further experiments being made with this drug in this connection.

Reviews.

BRIGHT'S DISEASE OF THE KIDNEY. By Alfred L. Loomis, M.D., LL.D., etc.
Detroit, Mich.: Geo. S. Davis, 1888.

The author defines Bright's disease "as a non-suppurative inflammation of the kidney, with manifestations determined by its etiology, the special elements affected, and the inherited tendencies of the patient." The definition strives to do away by its general and comprehensive character with the difficulties surrounding the name unfortunately bestowed upon many rather than one disease. By strict pathological classification the name should, the author thinks, be restricted, as by Virchow's original distinction, to parenchymatous and interstitial nephritis, but from its clinical significance it has become customary to include "the waxy, lardaceous, or amyloid kidney under this head." And the author further remarks, "All pathological changes, except the amyloid, not only may be, but usually are, present in varying proportions in every case of Bright's disease"

The author divides his subject into parenchymatous and interstitial nephritis and amyloid kidney. As throwing light upon the author's views of a vexed question, we quote the following: "Cases of Bright's disease which are chronic from their inception will be found to develop in two classes of subjects,—first, those suffering from the fibroid diathesis, in whom all forms of irritation tend to the induction of fibroid inflammation not only under stimulation which in other individuals would produce only healthy nutritive activity; second, those in whom, either through an inherited or acquired functional weakness of the organs of direct and retrograde metabolism, or on account of intemperance in food or drink, the kidneys are for a long time called upon to eliminate both an excessive quantity of the normal elements of excretion and products of faulty metabolism." By a combination of these two classes we may find "a set of individuals . . . who combine a marked fibroid diathesis with functionally weak liver, heart, and mucous membranes, and for whom we can predicate death from either apoplexy, cardiac failure, or Bright's disease." In such simple and easy language does the author dispose of a troublesome question, shoving the difficulty along by means of such machinery as "fibroid diathesis," "fibroid inflammation," and "faulty metabolism." The difficulty still, however, remains to tempt future investigators.

The question of uræmia the author considers a mixed one, urea retained in the blood being the most important of perhaps many self-generated poisons, the toxic condition being the product of all rather than of any one.

Of albuminuria it is remarked, "Within a comparatively short time albuminuria has ceased to be synonymous with Bright's disease," and pathologists have come to recognize a physiological and pathological albuminuria. Of the subjects of physiological albuminuria, the author says, "Although by far the larger proportion of such cases terminate in recovery . . . I cannot but believe that they show a failure of cellular activity such as must strongly predispose to inflammatory and degenerative changes."

With regard to the question of increased arterial tension and cardiac changes of Bright's disease, the author, after a discussion of the views of Gull and Sutton, Mahomed, and others on the subject, says, "While high arterial tension throughout the body thus becomes the recognized cause of cardiac change, the explanations of its cause and nature are both various and contradictory." The theory of Cohnheim seems to him more satisfactory than any other, and cardiac hypertrophy in connection with Bright's disease, Dr. Loomis insists must be considered as either a necessary consequence, a compensatory process, or a physio-pathological condition connected with the renal disease solely through the sympathetic system.

The discussion of casts, retinal changes, etc., is full. After which the author takes up the special varieties of Bright's disease *seriatim*. The causation of acute Bright's disease the author attributes to sudden changes of temperature rather than cold in itself, especially in those susceptible, and direct local irritation by some foreign element in the blood. On the question of treatment, we read, p. 58, "We thus find several indications presented for consideration in the treatment of acute Bright's disease. The foremost is the relief of the kidney from both functional activity, and the irritation of its normal products of excretion,—viz., acid urine and partially converted elements of retrograde change. The former has received much attention, the latter hardly any, from clinical teachers and theoretical writers." Now, while we have searched diligently in the subsequent pages, we fail to find any new or novel suggestions which will satisfy the author's statement above, from which we had been led to expect some decided therapeutic advance on his part. Mineral waters, digitalis, saline diuretics, milk diet,—these are the means on which he, in common with thousands of other physicians, relies for local relief to the blocked-up and congested kidney.

In the treatment of chronic nephritis, the author believes that the nutritive processes should "be crowded to the utmost." Milk is his reliance, almost to the exclusion of everything else. When predigested "three, four, or more quarts" per day can be taken without excess. Except in cirrhotic disease, when be excepts the bichloride, he condemns mercurials, but begins iron early in the form of tinct. ferri chloridi.

Taken as a whole, Dr. Loomis has given in condensed form an exceedingly useful monograph upon the subject, and one that from its price is easily accessible, and cannot fail to enlarge the views of many upon a subject still in many points in doubt, but unstudied and misunderstood to a degree far beyond its pathological intricacy. E. W. W.

TREATISE ON THE DISEASES OF WOMEN. FOR THE USE OF STUDENTS AND PRACTITIONERS. By Alex. J. C. Skene, M.D., etc. With two hundred and fifty-one engravings and nine chromo-lithographs.
New York: D. Appleton & Co., 1888.

The author aims at bringing together the "fully matured and essential facts in the science and art of gynæcology," and many others seem to be actuated by the same aims. What claim Dr. Skene's book has, and how it compares with its many compeers, we will endeavor to discover.

The author divides the diseases peculiar to women into three classes,—from birth to puberty, from puberty to the menopause, and, thirdly, "those which come after the menopause." "The history of gynæcology, and the discussion of all unsettled questions, have been," thank heaven, "omitted."

The book itself is a credit to the publisher. It is illustrated very fully, and with many entirely new illustrations. The chromo-lithographs, nine in number, are marvels of the art, conveying a better idea of the author's idea than many pages of printed matter. In fact, all the illustrations are decidedly above the average, and some of them wonderfully vivid,—as an example, the very beautiful wood-cut on page 247 illustrating crescentic laceration of the os.

The ground covered by Dr. Skene's book is so essentially that covered by many recent works that an enumeration of its contents seems superfluous. Perhaps the most original matter is to be found in the chapters on "Diseases of the Urinary Organs." Here the author has improved very much on his already published and standard work on that subject, and, although covering in this volume much less space, we cannot but regard these chapters as superior to his previous work as a whole. The author has evidently thought more and experimented more, has had more cases since his work was published, and has improved in his methods of treatment.

The use of the endoscope or cystoscope has in this author's hands proved very useful. Dr. Skene's endoscope is simple in construction, and seems in his hands to have been of considerable service, but he acknowledges that the cystoscope of Nitze and Leiter "is the only instrument for thoroughly investigating the bladder." This he very fully describes, with illustrations, quoting from Dr. Willy Meyer's article in the *N. Y. Med. Journal*, April 21, 1888, at considerable length.

In the chapter on "Organic Diseases of the Bladder," the author gives a very interesting treatise on bladder hemorrhage. In regard to blood-clots, he says, they "should be allowed to remain until they break down and come away of themselves. When actual inability to urinate persists, from the presence of a large coagulum, the author advises a flexible catheter carefully passed through or around the clot with as little disturbance of the clot as possible. The section on "Acute and Chronic Cystitis" is very valuable, and the treatment recommended by the author is sound. In fact, the really important part of the work is comprised in the section on "Diseases of the Urinary Organs," occupying over three hundred pages. Here the author is completely at home with his favorite subject, and has given us reason for saying that his treatise on the diseases of women possesses some peculiar claim to the favor of the medical world. E. W. W.

ELEMENTS OF PRACTICAL MEDICINE. By Alfred H. Carter, M.D., London, etc. Fifth edition.
London: H. K. Lewis, 136 Gower Street, W. C., 1888.

This is one of those condensations which owes its being to the demands of the student, and affords a means of more rapidly acquiring that kind of knowledge which suffices in technical examinations. These over, and its work may be considered as completed. The bold facts of the subject are all there, without any of the graces of rhetoric to attract the mind and stimulate the memory. Probably there are some to whom this book may be of use after examinations are over, but they must belong to that rare class who read the directory for amusement, and find a mental solace in Webster's dictionary. E. W. W.

SECOND SERIES, PHOTOGRAPHIC ILLUSTRATIONS OF SKIN-DISEASES. By Geo. Henry Fox, A.M., M.D., etc. An atlas and text-book combined. Complete in twelve parts. Hand-colored plates. Nearly one hundred cases from life. Parts I. to VI.

These extremely valuable plates have now reached the sixth number, and continue to display the same high standard of artistic excellence. The parts already published cover, with the associated text, seborrhœa, erythema, varicella, and variola, urticaria, the different forms of dermatitis, eczema, sycosis, psoriasis, lichen, pityriasis, and herpes. To the many who are unable to pursue the subject of skin-diseases in any other way, they will prove invaluable, and will enable any careful observer after their study to diagnose cases with almost as much readiness as from the actual observation of many cases in real life.
E. W. W.

HANDBOOK OF MATERIA MEDICA, PHARMACY, AND THERAPEUTICS. COMPILED FOR THE USE OF STUDENTS PREPARING FOR EXAMINATION. By Cuthbert Bowen, M.D., B.A., editor of "Notes on Practice."

Philadelphia: F. A. Davis, publisher, 1888.

The author wisely tells us for whom he has prepared this work, but we are all students and all subject to examination; and the many facts which he masses together, and his system of question and answer which he pursues throughout the book, lend it an additional interest to the reader surfeited with didacticism. Whether all the questions are pertinent, or all the answers correct, we cannot say. All we have noticed are, and the book seems carefully prepared. It contains many formulæ, some of them quite new to us and of seeming value, and for the student in the anxious stage of preparation we should imagine it a very efficient help. E. W. W.

COMPENDIUM DER ARZNEIVERORDNUNGSLEHRE FÜR STUDIERENDE UND ARZTE.[1] Von Professor Dr. Rudolf Kobert, Director des Pharmakologischen Institutes zu Dorpat.

Stuttgart: Verlag von Ferdinand Enke, 1888.

The brochure of two hundred pages which lies upon our editorial table has apparently for its chief aim the teaching of medical students and physicians such portions of pharmacy as may be especially valuable to them. It consists of the discussion of the world's pharmacopœias; of the method of application of drugs by physicians; and an elaborate account of the various classes of preparations employed.

It is without doubt a valuable aid to Professor Kobert in his teaching of Russian students in Dorpat; but as it is in the German language, and especially as it has close relations with continental rather than English pharmacopœias, it is of apparently little interest to American or English readers.

ARBEITEN DES PHARMAKOLOGISCHEN INSTITUTES ZU DORPAT. Herausgegeben Von Professor R. Kobert. Stuttgart: Verlag von Ferdinand Enke, 1888.

The production of two such volumes as these now before us as the result of original work in one laboratory in a single year is a notable event in the history of pharmacological advance. The three hundred pages of the two brochures are chiefly occupied with the record of experimental work performed,—work based, however, upon a full knowledge of the labor of others. The list of the papers is as follows:

Vol. I., "Ueber Sapotoxin," von Dmitrij Pachorukow; "Ueber Senegin," von Joseph Atlass; "Ueber Cyclamin," von Nicolai Tufanow. Vol. II., "Ueber die Wirkungen des Chroms," von Heinrich Pander; "Ueber Cytisin," von Raphael Radziwillowicz; "Vergleichende Versuche über die giftige Wirkung der Gallensäuren," von David Rywosch.

Dmitrij Pachorukow reaches the conclusion that sapotoxin is a violent poison for all portions of the nervous system, as well as for the circulation, and for both striated and non-striated muscle, and for the red corpuscles; but that the decoction of the bark of the quillaia as an expectorant is equal to that of senega, that its disagreeable qualities are not greater than those of senega. We would likewise say amen to the banishment of both quillaia and senega from the materia medica as substances of uncertain value in respiratory therapeutics, more than unnecessary in the richness of our modern materia medica list.

As concerns senegin, Mr. Atlass comes to the conclusion that while in its physiological action it is entirely similar to that of sapotoxin, there is a great difference in the degree of activity, and that the two substances are not, therefore, physiologically absolutely identical.

The study of the active principle of the Alpine violet leads friend Tufanow to the conclusion that a mixture of its active principle, cyclamin, with milk, causes every particle of the fat of the milk to separate in the course of ten or twelve hours, and come to the surface; that this cyclamin belongs pharmacologically to the saponine group, which included quillaiac acid, sapotoxin, senegin, digitonin, and smilacin; that the cyclamin is not of the nature of a ferment, and that it has the power of acting locally upon the walls of the blood-vessels both in cold- and hot-blooded animals, so as to cause contraction of their lumen and rise of the blood-pressure, followed by a fall if the dose has been large enough; that it acts directly upon the muscular substance, not only of the heart, but also of the voluntary muscles, so that the myosin coagulates, and that it is of very little, if any, use in practical therapeutics.

Heinrich Pander concludes his elaborate study of the action of the preparations of the chromium salts, with a tabulated collocation of forty-three cases of poisoning, and, by a comparison of the action of chromium with the other metals, reaching the conclusion that of all the metals only the silver in the form of an oxide is as inert as is chromium oxide. It seems evident that in the numerous cases of poisoning caused by chromic acid the symp-

toms have been due to the irritating or corrosive effect of the drug.

Raphael Radziwillowicz believes cytisin to be between strychnine and curare in its pharmacological relations, but to still more closely resemble in its action ulexin. He believes that it is generally capable, in doses of one milligramme, given subcutaneously, of increasing blood-pressure without influencing the heart, and that its use is indicated in all cases where there is a low blood-pressure and relaxation of the vessels.

The paper upon "Wirkung der Gallensäuren," by David Rywosch, points out the poisonous qualities of the bile and its acids, and is more of interest to the pathologist and toxicologist than to the therapeutist. We congratulate Professor Kobert on the result of his laboratory-work during 1887, and trust that the year 1889 may give to us a still more interesting record of work done in 1888.

Correspondence.

CONTRIBUTIONS TO THE DISCUSSIONS ON THE DIAGNOSTIC VALUE OF THE TOLERANCE OF THE IODIDES IN SYPHILIS BETWEEN J. WILLIAM WHITE, M.D., AND H. C. WOOD, M.D.

By Professor James Stewart, Montreal; Professor James Nevins Hyde, Chicago; Professor Fessenden N. Otis, New York; Dr. John P. Bryson, St. Louis; Dr. John B. Chapin, Philadelphia; Dr. Edward N. Brush, Philadelphia; Professor R. W. Taylor, New York; Professor F. R. Sturgis, New York; Professor McCall Anderson, Glasgow; Professor Edward L. Keyes, New York; Professor Roswell Park, Buffalo; Professor William Osler, Philadelphia.

Dr. H. C. Wood, Editor of the Therapeutic Gazette:

Dear Doctor,—In the December number of the Therapeutic Gazette were published my paper on "The Diagnostic Value of the Tolerance of the Iodides in Syphilis," your reply, under the title of "The Iodide of Potassium in Syphilis," and my further comments, "Iodism and Syphilis," which closed the discussion.

As the same number of the Gazette contained an editorial request for opinions upon the subject, it occurred to me that an expression of the views of a number of well-known physicians, with special experience in regard to the question at issue, would be of interest to all those who were already familiar with the opposing theories.

I accordingly wrote to fifteen gentlemen, as follows:

"1810 South Rittenhouse Square,
"Philadelphia, December 28, 1888.

"My dear Doctor,—Will you do me the favor, after reading the pamphlet which I send you by this mail, of telling me your opinion as to the question at issue? And, if you can spare the time, will you kindly give me your reasons for that opinion? I intend to publish the replies received from a dozen of the best-known syphilographers and neurologists of the country.

"I need scarcely say that I want to hear from you whether you agree with my views or not. You will greatly oblige me if you can send me an answer at an early date.

"Please believe me, with sincere respect,
"Yours very truly."

In reply I have thus far received the twelve communications which I herewith enclose, and which I send you for publication.

Very sincerely,
J. William White.

February 12, 1889.

"Montreal, January 10, 1889.

"My dear Doctor,—I have been much interested in the important questions discussed so ably by Dr. Wood and yourself in the Therapeutic Gazette for December.

"From a careful perusal of Dr. Wood's paper I come to the conclusion that he maintains the following propositions as practically proven:

"I. That in nearly all cases of chronic syphilis, especially in syphilis of the nervous system, the administration of the iodide of potassium is not attended by iodism in any of its forms.

"II. That the administration of the iodide of potassium in health and in diseases other than those of a syphilitic character is almost universally attended by iodism.

"I have always looked upon iodism occurring in the course of the iodide-treatment of syphilis as exceptional, the rule being that it is far from common to find iodism, but the exceptions to this rule are, in my opinion, numerous enough to take away all value from the proposed 'therapeutic test.'

"It appears to me that iodism is simply a proof that the elimination of the drug is to some extent effected by the skin and mucous membranes, and that the tolerance of large

doses for long periods is explained by the eliminating surfaces becoming accustomed to the irritating effects of the drug. If any part of the cutaneo-mucous tract is the *locus minoris resistentiæ*, we have symptoms of irritation of that part.

"As a rule, to which, however, there are no doubt many exceptions, the more active the draining powers of the kidneys, the less likely is there to be iodism, because there is less elimination through the skin and mucous membranes. The less the elimination through the kidneys, the greater the danger of iodism. There is abundant clinical proof for the last statement.

"Dr. George Johnson* has especially called attention to the frequency of iodism in cases of chronic Bright's disease.

"There can be no lack of evidence to prove the truth or falsity of the assertion,† that 'the vast majority . . . of persons who are free from syphilitic infection cannot take doses of over 10 grains of the iodide three times a day without the production of iodism, unless as the result of the habitual use of the remedy. When iodides are tolerated by the normal individual, such individual is said to have an idiosyncrasy, which makes him an exception to the general rule.'

"The action of the iodides in health, or in those who have some temporary or trifling complaint, is, in my experience, mostly a matter of dosage.

"When given in doses of 1 to 3 grains three times daily, running at the nose almost invariably is seen; but if the quantity be increased to 20 grains and upward three times a day, the coryza at once ceases. I am unable to offer any explanation of this marked difference in the action between large and small doses. Brunton‡ refers to this difference, and I believe that it is a very common experience.

"I have had no experience of the action of very small doses in chronic syphilis. I have no doubt that such a test would be instructive, and would possibly throw some light on the questions in dispute.

"There are two diseases where the iodides are usually given in considerable doses and for lengthened periods, and where there is, as far as I am able to determine, the same degree of immunity from iodism as there is in syphilis. I refer to aortic aneurism and chronic lead-poisoning. Dr. George W. Balfour, of Edinburgh, who probably has had more experience with iodide of potassium in aortic aneurism than any living author, says,§ that he has only twice seen cases of complete intolerance of the iodide.

"Dr. George Ross, of Montreal, who has treated a large number of cases of aortic aneurism with iodide of potassium, says that it is a very exceptional occurrence to find any symptoms of iodism.

"It must be admitted that in a certain proportion of cases of aortic aneurism the patient is syphilitic.

"I have treated, during the past five years, upward of twenty cases of chronic lead-poisoning, and in not one instance is there any record of any iodism, and, with the exception of one case, there was no history of syphilis.

"I believe that my experience of the subject under consideration warrants me in coming to the following conclusions:

"I. That the exceptions to the rule, that iodism is very rare in syphilis of the nervous system, are too numerous to admit of the proposed 'therapeutic test' as being of value.

"II. That in certain other diseases (aortic aneurism and chronic lead-poisoning) treated with iodides, we have as pronounced an immunity from iodism as we have in syphilis.

"III. That in health iodism is the rule only when very small doses are taken, while large doses quickly arrest the iodism induced by the small doses.

"IV. That the toleration of large doses (10 to 20 grains) in health is the rule and not the exception. The advent and *not* the absence of iodism proves the idiosyncrasy of the individual.

"JAMES STEWART,

"*Professor of Therapeutics and Lecturer on Diseases of the Nervous System in McGill College, Montreal.*"

"I have read with great interest the two contributions to the subject of the diagnostic value of the tolerance of the iodides in syphilis made by Drs. J. William White and H. C. Wood, of Philadelphia. Being asked for my view of the questions upon which they fail to agree, it is needful to admit at the outset that one is justified in speaking with hesitation on any point in controversy between two col-

* Quoted by Lewin in his work "Untoward Effects of Drugs," p. 111.

† Wood, H. C., THERAPEUTIC GAZETTE, December, 1888.

‡ "Pharmacology and Therapeutics," 3d ed., p. 253.

§ "Diseases of the Heart," 2d ed., p. 459.

leagues of such distinguished position and large experience.

"A personal quality in the statements which follow is almost unavoidable. My written records cover only about fifteen years of experience, both in private practice and in civil and military hospitals, the cases of patients having nervous complications of the disease being not separated from the thousands exhibiting other symptoms of syphilis. Of immediate practical value in this connection is the fact that during the last five years, both in private and consultation practice, nervous syphilis in all grades from milder to grave and even fatal cases, has become disproportionally frequent.

"It is proper to admit here that the more highly specialized a man's experience, the less competent he becomes, not necessarily but presumably, for a decision of questions involving comparisons between the subjects of such specialized and non-specialized study. In this view, Dr. Wood, it seems to me, has some advantage over both Dr. White and myself. Dr. Wood appeals, as ground for acceptance of his views, to a hospital experience in fifteen years of more than two thousand patients, between twenty-five and fifty per cent. of whom were victims of non-syphilitic nervous disease. During the same period of time, while not able to speak for Dr. White, I believe that my personal experience in non-syphilitic nervous disease has been in comparison with these figures not worth citing.

"Under the comprehensive and acceptable term 'iodism' are now included and accurately tabulated an enormous list of symptoms of iodic intoxication exhibited in almost every system and viscus of the body, including what may be termed a mimicry of almost every cutaneous disease in our nomenclature. No ampler and more exact exposition of the subject has been made than in the monograph entitled 'L'Iodisme,' written by Dr. Elizabeth Bradley, and published in Paris in 1887. In this excellent work are included all the cases referred to by Dr. Duffy, cited by Dr. White. Two of my personal memoirs on the bullous rash induced by the iodides are here summarized. A few contributions to the subject which have appeared at a later date are, of course, wanting,—*e.g.*, Hallopeau's 'Report of Singular Cicatriform Lesions following an Iodide Exanthem;' Taylor's paper (read before the First Congress of Physicians and Surgeons in Washington in 1888) on so-called 'Acne iodo-potassique Anthracoïde;' and my paper on 'Dermatitis Tuberosa due to Ingestion of the Iodine Compounds,' read before the same body at the same date with Dr. Taylor's.

"Recognizing, then, the very wide range of phenomena included under the term 'iodism,' Dr. Wood's position may be tersely stated as follows:

"*a.* The majority of persons in health cannot tolerate large doses of the iodides.

"*b.* A large number of persons in health can secure such toleration by careful graduation and increase of the dose.

"*c.* A notable exception may be found in a small minority of persons in health who enjoy complete toleration as the result of idiosyncrasy.

"The reverse of these propositions is also held to be, in the main, true; thus:

"*a.* The majority of syphilitic persons tolerate large doses of the iodides.

"*b.* Careful graduation and increase of the dose in syphilitic patients does not interfere with toleration.

"*c.* A notable exception may be found in a small minority of syphilitic persons who cannot tolerate the iodides as the result of idiosyncrasy.

"From these propositions it is inferred that when in any person toleration of the iodides in large doses is exhibited, the diagnosis of syphilis is warranted if symptoms are also present of obscure organic disease. In another paragraph Dr. Wood indicates that failure of production of iodism by sixty grains of the iodide of potassium ingested daily practically establishes a diagnosis of syphilis.

"As to terms here used, it is understood and agreed that for purposes of the present question the 'syphilis' of Dr. Wood's paper is nervous syphilis, belonging to the advanced, late, or gummatous stage of the disease, which, it is well understood, may occur in special cases speedily after infection.

"Dr. White urges effectively, *per contra*, that the indications for the free use of iodine do not exist until this advanced or later stage of the disease has been reached, when the blood and tissues are not demonstrably pervaded by an active and generalized virus; when this virus has become non-symmetrical, non-contagious, and in many cases non-transmissible; when, in point of fact, there is no virus which a poison can neutralize. In this he is in full agreement with most syphilographers, and might even have added, that a coarse test of any physician's expertness in the management of the disease is here

furnished, most general practitioners hastily employing the iodide at periods when the expert finds no indication for its use. But the impressive fact is not to be forgotten, that while not demonstrably in its later or even last stage a generalized disease, still, after syphilitic infection, some general and persisting effect has surely resulted upon the system at large, since a new infection is, for the immense majority of all patients, impossible. No one can verify the exactness of Colles's law in a series of years and cases without reaching the clear conviction that the mother of the syphilitic child, even though herself exhibiting no symptoms, symmetrical, contagious, or transmissible, even though under the influence of no demonstrable virus, yet exhibits completest and fullest immunity as against the infectious mouth of her syphilitic offspring.

"It must also be freely admitted in Dr. Wood's favor, that toleration of the iodides by the syphilitic patient by no means indicates relief of syphilitic symptoms, that many cases of severe nervous syphilis are benefited immediately by mercury when the largest doses of the iodides are apparently without value, that the sequelæ of syphilitic lesions may be as serious in results as those immediately due to the lesions themselves, and that sclerosis in syphilitic subjects is certainly not amenable to specific treatment. This much admitted, I find myself totally unable to agree with this eminent author that there is any antagonism between the action of the poisons of syphilis and the iodine compounds to an extent justifying diagnostic conclusions. For,—

"I. We may entertain the suspicion that if we only knew enough there need be no intolerance whatever of the iodides by any person. The mischief is, we do not know how to establish toleration for some patients, while we do know for some others how to compass this end. Any conclusions based on different methods of administering the drug in different periods of the life of one person must be practically worthless. Given a uniform method and uniform conditions, and the number of idiosyncratic patients becomes rapidly smaller. Of this I am thoroughly convinced by experience. A few years ago I began to use only one method of administering the iodides, and by this term it may be conceded that all parties to the discussion intend to designate chiefly the iodide of potassium. That method was to employ a saturated solution, one grain to the minim of distilled water, given in milk, when that vehicle was not found to disagree as an article of diet with the patient to be treated. The dose was made, at the outset, not more than 5 to 10 drops; the increase was ordered every three or four hours; and in face of the chemical dictum lately set forth, that the drug should always be given fasting, it was ordered after a small quantity of food was ingested. In this way the larger doses, as high as eight hundred grains daily, have been reached. I note, in passing, that it is my conviction that the very largest doses reached have not seemed as valuable as those which may be described as 'large,' the majority of all brilliant results being attained, I think, by a dose within rather than beyond two hundred grains daily.

"Observance of a uniform method in scores of cases soon led to the conclusion that 'idiosyncratic' patients were far fewer than was at first supposed. Careful graduation of the dose, and elimination of the cases in which the patient, eager for aid, surreptitiously swallowed more than his dose, threw new light upon this question. Again, the marked idiosyncratic exceptions that were observed soon occurred with symptoms of intoxication not cutaneous in type. Violent abdominal pains, tumefaction of the belly, grave prostration, and indefinable nervous impressions, which patients found difficulty in describing, but which they referred to with evident horror,—these were of the class that proved exceptions to the rule. The symptoms of iodism strictly cutaneous in type and of the odd appearance which I have described in my papers, were of the sort generally to which Dr. White refers,—that is, suddenly occurring at the outset of administration of the drug, in non-syphilitic (often very young) patients treated somewhat awkwardly by general practitioners for eczematous and other diseases of the skin. Let me note, in passing, on this part of the subject, that of these cutaneous rashes, one, I am satisfied, is etiologically separated from all the others. Whether the others be reflex, vaso-motor, or tropho-neurotic phenomena, or whether they be in part due to abortive efforts at elimination of iodine by the emunctories, it is certain that severe forms of purpura, even at times alarming in grade, may ensue long after full toleration has been established, being displayed most conspicuously in the lower extremities and over the belly. This, there can be but little question, is due to weakness of the vascular capillaries

(probably induced by both syphilis and iodine intoxication long continued), a condition in the adult not greatly different from some of the reported forms of hæmophilia due to inherited syphilis.

"II. The fact that most patients exhibiting the symptoms of nervous syphilis are in the early periods of adult life, when the tissues are far more tolerant of toxicants than in youth, late middle life, and old age, is not only in part explanatory of the fact urged by Dr. Wood as evidence of syphilis, but is to be considered in connection with

"III. The further fact that most victims of nervous syphilis are of the male sex. The reasons for this are so evident that they need not be enumerated. If, now, we compare the men in the vigor of this period of life, mostly in the third decade, stricken with nervous syphilis at the date of the fullest enjoyment of sound health for the average of their race, possessing good average powers of resistance to poisons, and having relatively coarse skins, with the twenty-five to fifty per cent. of Dr. Wood's hospital patients having non-syphilitic nervous disease, what do we find? We may assume that Dr. Wood's patients were of the class that other physicians find in other cities than Philadelphia, similarly disordered. He probably had his full share of cases of infants and children affected with infantile spinal paralysis, chorea, tubercular meningitis, hydrocephalus, convulsions, and other nervous disorders of that period of life. Certainly, as regards cutaneous symptoms of iodism, this furnishes, in my experience, the most favorable class for such exhibition. He probably includes in his statistics the cases of hystero-epilepsy and hysteria in women, more particularly in young women, and those of catalepsy with other affections of the nervous system peculiar to that age and sex. Surely we may consider these girls and young women with delicate skins as second in the list most liable to iodic cutaneous manifestations, little apt to enjoy tolerance of the drug. Lastly, we may name the senile patients with *ramollissement*, paralysis agitans, and similar disorders, including those in the fourth decade of life or later, exhibiting symptoms of *délire de grandeurs*. No one of the classes here suggested includes patients whose tissues have such power of resistance to poisons as those of our average male patients with signs of nervous syphilis.

"Lastly, IV., and this point has been touched upon above, the suddenness of attack of nervous syphilis in our average male patient in the second decade of life brings him before his physician with a good reserve of vital energy to draw upon in an emergency. Possibly this furnishes in part the excellent statistical results of treatment in cases early recognized. If he lie in bed, apparently near his end, but actually destined to survive the natural expectation of longevity for his age, he has often a torso that might serve for a sculptor's model. He may drag a lower limb that would not deform a statue of Apollo, or, when taxing the English language for terms in which to eloquently describe his agonies of the night, exhibit a face that to all but the eye of the expert might pass for that of one in sound health. I have often been struck with the fact, that the average patient exhibiting marked syphilitic cachexia is not of this class. The cachectic is often the hospital patient presenting the most repulsive of the pictures in the gallery of deformities producible by lues. He may have what the farriers call a 'splint' on each leg from osteo-periosteal disease; his pinched and sallow features tell a pitiful story of malnutrition; the nose may be simultaneously flattening from caries of its bones; he may also exhibit half a dozen deep and formidable ulcerations from degenerating gummata,—and yet not suffer from any one of the many signs of nervous syphilis that may be fully shown in the next well-nourished patient under observation without an external stigma of disease.

"I conclude with the remark, that if Dr. Wood should come to me with the statement that he had under observation a vigorous male patient in the second decade of life, with an obscure disease of the nervous system, who was tolerating large doses of the iodide of potassium so well that a diagnosis of syphilis was practically established on that basis alone, and that the reason for the tolerance was an antagonism between the virus of syphilis and the poison of the iodide, I should respond: 'Granting the exactness of your hypothetical diagnosis which, in any event, I regard as well placed, one is justified in believing that your patient tolerates well the remedy you have employed, because he is what he is, syphilis wholly excepted.'

"JAMES NEVINS HYDE,
"*Professor of Dermatology and Venereal Diseases in Rush Medical College.*
"CHICAGO, January 5, 1889."

"NEW YORK, 5 WEST FIFTIETH STREET,
"January 10, 1889.

"MY DEAR DOCTOR WHITE,—In answer to your request for my opinion in regard to the diagnostic value of a toleration of large doses of the iodide of potassium in determining the presence of syphilis, and also the reasons for that opinion, I reply to the first by an expression of my belief that such toleration is wholly valueless as a proof of the present or antecedent existence of syphilis in any case. As to the reasons for this opinion, I wish to say that, during an experience of over thirty years in the study and treatment of syphilis, I have met repeated cases where the iodide of potassium has been administered up to a drachm three times a day with easy tolerance in individuals suspected of having syphilis, but in whom there was no positive evidence of the existence of the disease in its early or late forms, and where the subsequent history confirmed this position. I have also myself, in testing cases where the diagnosis was doubtful as between cancer and syphilis, repeatedly found the highest tolerance in cases which subsequently proved to be cancerous. On the other hand, I have met many cases of unquestionable syphilitic origin where the most marked intolerance of the iodide of potassium was present; where from 5 to 10 grains, thrice daily and largely diluted, would promptly produce some form of iodism. In one case in particular, which I shall not readily forget, 10 grains of the drug, thrice daily for a week, produced œdema of the pharynx and glottis, which nearly proved fatal. I have at the present time under observation two cases, who have an undoubted syphilitic history, where any attempt to raise the dose above 10 grains produces coryza and indigestion within a few days to an extent making its continuance unbearable; and again another, where 5 grains produce an unpleasant aching in the parotid and submaxillary glands within an hour. I have also seen repeated cases of grave cerebral trouble where a claimed intolerance of the iodide of potassium has been the basis of a diagnosis excluding syphilis, and yet where, by varying the methods of administration of mercury and the iodide of potassium previously pursued, these remedies have been tolerated to the extent of producing manifest improvement, and in some cases even cure. Prominent among my records is a case coming to me from a distinguished specialist in mental diseases, where, because of the apparent intolerance of both mercury and the iodide of potassium, the case was pronounced one of the melancholic forms of general paralysis of the insane, non-syphilitic, and hence hopeless. Under a more careful and persistent effort, resulting from positive knowledge of a previous syphilitic infection, a toleration of both mercury and the iodide of potassium up to the full physiological effects of each resulted in rapid and apparently complete recovery,—the patient being now (January 10, 1889) in perfect bodily and mental condition, and having been so for nearly two years.

"A few words in regard to the reasons which seem to me to support the results of my clinical observations. The late lesions of syphilis are especially in question. Professor Wood says, on page 12 of your discussion, that '*the tolerance of the iodides* belongs to the advanced rather than to the early *stages* of the disorder.' The late lesions of syphilis (sequelæ) can, I think, be reasonably shown to be always *local* in their nature and development, and always to begin by a deposit of the so-called 'gummy' material. This scientific investigation has shown to consist chiefly if not wholly of germinal material, and in no way different from that of normal origin.* In further proof of this position is the fact that, under favorable circumstances, it is capable of organizing into fibrous or cicatricial tissue. Subjects of gumma are ordinarily quite well, except from the mechanical effects of its presence. The children they beget are free from any syphilitic taint.† The removal of the gumma, wherever situated, may be effected by the efficient use of the iodide of potassium alone, resulting in a prompt disappearance of symptoms, and, if the removal of the deposit is complete, there is likely to be no return. But if more or less sclerosis has occurred, the use of the iodide is found to be inefficient. This agent seems not to be of sufficient power to produce fatty degeneration, to remove the deposits after a certain degree of organization has taken place. In such cases mercury is found of greater efficiency,—not because it is an antidote to syphilis, but because it is the most powerful of all known agents in producing fatty metamorphosis. No increased tolerance of mercury is claimed in persons the subjects of syphilis. The physiological effect of

* See "Cornil on Syphilis," translated by Professors Simes and White, page 206. Lea & Co., Philadelphia, 1882.

† See Fournier, "On Syphilis and Marriage," page 231. Paris, 1880. Also Otis, "On Syphilis and the Genito-Urinary Diseases," page 171. Putnam, New York.

mercury consists simply in a more or less pronounced fatty degeneration of healthy tissues, first observed in the mouth and adjacent gland-structures. This occurs quite as promptly in the syphilitic as in the non-syphilitic. There would appear, then, to be no especial reason why the iodide of potassium, which apparently acts upon the late lesions of syphilis practically in the same way, should be credited with a salutary antagonism to a poison which is fairly well proven to have no existence in the so-called tertiary or late stages of syphilis. I feel confident that further careful observation of the nature and treatment of syphilitic disease in its various stages and sequelæ will show that the antidotal view of remedies in syphilis is untenable, and that the medicines and measures which prove most useful in the treatment of this disease will be found in no sense peculiar in their effects upon persons who have, have had, or who have not had syphilis.

"Very truly yours,

"FESSENDEN N. OTIS,

"*Professor of Genilo-Urinary Diseases in the College of Physicians and Surgeons, New York.*"

"DEAR DOCTOR WHITE,—A careful reading of the pamphlet sent me leads to the inference that the 'question at issue' referred to in your note of December 28, 1888, and concerning which you request my opinion, is fully embodied in the following sentence quoted by you from Dr. H. C. Wood: 'In all cases of doubtful diagnosis the so-called therapeutic test should be employed, and if sixty grains of the iodide of potassium per day fail to produce iodism, for all practical purposes the person may be considered to be a syphilitic.' Now, if I am asked whether I consider this broad rule of action sufficiently reliable to form the basis of a diagnosis, I reply that I do not. I believe that its application in practice in doubtful cases would lead to error in two directions:

"1. By leading to a diagnosis of syphilis when that disease was not present; and,

"2. By failure to diagnosticate the disease when it was present.

"And, furthermore, I believe that either of these errors would occur so frequently as to seriously vitiate the result. The fact that they lead to error in opposite directions doubles the ultimate effect and all the more confirms me in the opinion that the rule itself is *fallicissima res.*

"It seems hardly necessary to say that my belief is based upon my observations of the frequency of the occurrence of iodic intoxication ('iodism') in cases of late—that is, the so-called tertiary—syphilis. Only exceptionally do I give the iodides in the earlier manifestations, and this for the reason that they are not then needed; but I do give them, and find that they produce 'iodism' as frequently as in the later stages, but not more so. In what I have to say here in relation to the value of the non-appearance of 'iodism' as a diagnostic of the existence of syphilis, let it be understood that I confine myself to the employment of that drug in the later stage. Also, let me say that, for present purposes, the term 'idiosyncrasy' is held to mean an individual peculiarity which causes a certain proportion of our patients to exhibit symptoms of the toxic effect of the drug when given in moderate or small doses, while 'iodism' should include 'the various forms of iodic intoxication, the most common of which are gastro-intestinal irritation, coryza, lachrymation, mental depression, tinnitus aurium, and a pustular eruption (acne) upon the cutaneous surface.' With this understanding, I am quite within bounds when I say that fully fifty per cent. of the cases of late syphilis treated by me exhibit iodism long before sixty grains per day is reached. In fact, it is only in a minority of the cases that *some* of the toxic effects fail to show themselves before a dose of thirty grains per day is reached. If we include all the untoward effects of the iodides which we see in the eyes, nose, pharynx, fauces, mouth, stomach, skin, and nervous system under the term iodism, I cannot make out that the size of the daily dose bears any relation whatever to their appearance. So far as I can see in practice, a given patient who bears 5-grain doses thrice daily, without any toxic effect, will bear equally well 20-grain doses repeated at the same intervals. My own observation, then, accords with the assertion of Jonathan Hutchinson, that the size of the dose bears no relation to the frequency of the toxic effects. The most dangerous case of iodism which I ever had in my practice followed quickly upon a 3-grain dose. When an important viscus (brain, liver) is threatened, or when any organ is having serious damage wrought in it by syphilis, I do not suppose that any one would be deterred from pushing the remedy by the occurrence of a minor evidence (acne, coryza) of iodic intoxication. And, perhaps, after all, here lies the reason for the great

discrepancy of evidence in this matter. Patients often overlook the minor evidences until attention is called to them by the physician; and it seems to me to go without saying that cases of cerebral syphilis would be vastly more apt to do this than any others.

"That there is wide difference of opinion in regard to the relative tolerance of the iodides by the syphilitic and the non-syphilitic, as well as in the views of different observers and investigators as to the means to be resorted to to determine the question, there can be no doubt. Upon one point, however, there is practical unanimity of opinion, as indeed there could not help being: this is the fact that 'iodism' does appear on the administration of moderate, even small, doses of the drug to patients who we know have syphilis in the later stages. Here there is common ground upon which all may meet and fight the battle out, even if figures are to be the weapons. Meeting here, it will make no practical difference whether syphilis in any stage increases the tolerance of the iodides. The question will then be, whether 'iodism' occurs frequently enough in syphilitics to render the rule quoted above ineffective for diagnosis. I believe it does. If this view is the correct one, then any discussion of the question of tolerance, highly interesting as it is, is of no practical use in the determination of the diagnostic point. If the question of this 'idiosyncrasy' can be eliminated by reason of its infrequency, then the matter of relative tolerance will be of high value.

"Figures as to this frequency of iodism in syphilitics I confess my inability to give, for the reason that neither at my clinic nor in my private practice have cases been recorded with this question in view; and I do not believe that the mere relation of such few cases as I now have in mind would accomplish the object sought.

"Very sincerely yours,

"JOHN P. BRYSON,

"*Fellow of the American Association of Genito-Urinary Surgeons.*

"ST. LOUIS, January 28, 1889."

"PENNSYLVANIA HOSPITAL FOR THE INSANE,

"PHILADELPHIA, January 25, 1889.

"DEAR DR. WHITE,—I have read the reprint of a discussion between yourself and Dr. H. C. Wood, taken from the THERAPEUTIC GAZETTE. The proposition which Professor Wood has announced is likely to attract attention, both on account of its novelty and because it is a very positive statement about a subject that is considered obscure and unsettled. If Dr. Wood's views turn out to be correct eventually, his present position certainly is that in which some of our legal friends find themselves,—with a good theory of law on their side, but not the facts to sustain it. The vigorous and temperate tone of the discussion is most commendable. A general response to the questions growing out of the discussion and the questions announced in the THERAPEUTIC GAZETTE cannot fail to throw much light upon this subject.

"Referring to exact data in my own experience, it may be stated that, at the present time, of three hundred and ninety patients in this hospital, one and a half per centum have such a history of syphilis that their insanity may be fairly attributed to that cause. From observation in hospitals for the insane outside of cities, the proportion does not seem to be as great, if we exclude paretics. In the production of paresis syphilis is as yet an unknown factor. The departments of a city almshouse hospital contain a population deteriorated by a low heredity and broken down by various vicious indulgences, that 'tolerate' medication to an extraordinary extent, as a rule. It is an element to be considered in deductions made from observations among this class, and might otherwise form a source of error.

"I recall five cases in which the insanity resulted from syphilis where the iodide of potassium was administered to the extent of forty-five and eighty grains daily. The medicine was discontinued because it produced gastro-intestinal irritation. In 1857 I reported a case of insanity from syphilis that recovered under the use of iodide of potassium in 5-grain doses three times daily. One case recovered in this hospital under the use of one hundred and eighty grains daily, and another made a good recovery taking eighty grains daily.

"So far as these cases go to establish any result, the majority show that extraordinary doses were not borne, and the medicine was discontinued, and in one case that a small dose accomplished as much as a large dose did in another case. But it may be said the diagnosis of these cases was not in doubt. It is conceded they were not, yet they show that a small dose and moderate doses sometimes accomplish the desired result as well as extraordinary doses; also, that the known existence of syphilitic disease did not prevent iodism, even in moderate doses.

"We are told, however, that in all cases of doubtful diagnosis, if the so-called therapeutic test is employed without producing iodism, the person may be considered a syphilitic. Does such a test commend itself to our best judgment as conclusive? Having administered large doses of a drug to a doubtful case, and not poisoned our patient or produced any other result, perhaps, are we to conclude he is a syphilitic? What has occurred to solve our doubts? Are our results not wholly negative, and is not an opposite conclusion equally legitimate? Is it true that in these tertiary cases we are contending against an active virus that is said to neutralize large doses of the iodide, or are we not rather seeking in our treatment to remove the results of the virus in its active stage, as we aim to remove the results of a meningitis, and exudations in other diseases? Cases of chronic rheumatism, obscure mental and nervous diseases, and meningitis occur which bear large doses of the iodide. Must we for this reason conclude they are syphilitic? If in a doubtful case the iodide cures our patient, our doubts may be said to be removed. But can it yet be said to be settled that the existence of syphilis was established? If we administer the iodide in a doubtful case and have no result and have not poisoned our patient, we have only established the fact that no idiosyncrasy exists,—perhaps nothing more. I cannot concede that a new principle can be said to have been evolved from what has yet appeared. If it has been and will stand the test of experience, it must have a wider application in the practice of medicine than that which has been made of it to the class of cases in question.

"Very truly yours,

"JOHN B. CHAPIN,

"*Physician in charge of the Pennsylvania Hospital for the Insane.*"

"PENNA. HOSPITAL FOR THE INSANE,
"DEPARTMENT FOR MALES, January 26, 1889.

"MY DEAR DR. WHITE,—I am under many obligations for the opportunity of reading the 'Discussion' between Dr. Wood and yourself.

"In answer to your request for my opinion upon the question at issue, I do not know that I can say anything which will add to the interest or value of the discussion so ably opened. At the same time, I must say that I have pretty firm and definite opinions based upon considerable experience with syphilis in its ordinary manifestations, as well as with so-called nervous syphilis.

"The late effects of syphilis, especially in the way of nerve-lesions, have interested me ever since my entrance upon the practice of physic. Indeed, my inaugural thesis was upon 'Syphilitic Affections of the Nervous System.'

"It seems to me that to base a diagnosis of syphilis upon the tolerance of large doses of the iodide of potassium, in cases in which the diagnosis was otherwise obscure and doubtful, would be unwise and at variance with the judgment and experience of the profession. I was long ago taught, in cases where the least doubt existed, and where its administration was not contraindicated by other conditions, to give the iodide, and to push it to the limit of toleration where such heroic treatment seemed desirable; but I have never ventured to diagnose syphilis, even in cases brought to a successful issue, because of the success; much less would I feel justified in so doing because of mere tolerance of any particular drug.

"If Dr. Wood means to say that few normal individuals (free from syphilis?) can tolerate sixty grains of the iodide a day, if at once placed upon that amount, I see no reason for differing materially with him; but still I am sure that there are numerous individuals who are, as far as syphilis is concerned, normal, who can rapidly be brought to take sixty, eighty, and ninety grains of the iodide a day. On numerous occasions I have prescribed at once 20-grain doses, to be repeated three times daily, and the instances in which any intolerance was shown were exceedingly rare. I cannot be induced to believe that all these cases who had a tolerance for the drug had syphilis; indeed, I know that many of them did not. It has been my habit for years, in prescribing the iodide, to commence at 10 grains, and, when desirable, to increase by adding one grain a day to each dose, the doses being given three times daily at least. The records of numerous cases which have occurred since my connection with this hospital do not show a single instance of iodism or anything pointing in that direction, except in one markedly syphilitic case (initial lesion in 1868), who had a profuse crop of acne after taking 10 to 13 grains three times daily for a few weeks.

"This case leads me to refer to another, in which there was a peculiar idiosyncrasy. I saw the patient, a medical man, in the spring of 1873, suffering from a neurosis of evident syphilitic origin. This patient could not take 20 grains of the iodide in solution three times

daily without having marked coryza, disturbance of digestion, etc., but he was able to take 96 grains in pill form three times daily for weeks with no ill effects.

"It may be answered that of these large doses a considerable proportion passed off by the bowel. The result proved the contrary, as rapid improvement occurred under the treatment by the pills, which had not taken place under its persevering use in solution. It would certainly have been wrong to have excluded syphilis because of the non-tolerance of the large (20 gr.) doses in solution.

"About the same time I saw a gentleman whose case had been progressively growing more serious, notwithstanding active and apparently judicious treatment. He had insomnia, great pallor, emaciation, and general malaise. There was no suspicion of syphilis, had never been any exposure, and the gentleman had no object in concealment; moreover, he knew the importance and bearing of the questions regarding syphilis which were put to him. His only heredity was a gouty diathesis. Under the use of the iodide of potassium, commencing at 20 grains, and rapidly increasing the dose to 60, and then to 90 grains, three times daily, this patient put on flesh, gained in color and strength, and eventually made a good recovery. The exact pathological condition was never diagnosed, but certainly syphilis must be excluded.

"I have administered, or seen administered, large doses of the iodides in cases of goitre, in certain rheumatic affections, in plastic pleurisy, in the treatment of aneurism after Balfour's suggestion, and in varied affections of the brain, chronic meningitis, tumor, acute hydrocephalus, etc., and while I cannot now refer to figures, I am confident that the tolerance of the drug was as great in non-syphilitics as in syphilitics. I speak with some confidence on this point, for the reason that long ago I established the rule of impressing upon persons whom I had treated for syphilis the importance of always in any subsequent illness informing the attending physician of the constitutional taint, and in cases which came under my care or observation with other diseases, I have been equally careful to inquire as to specific infection.

"In cases of insanity the history is often obscure, for the reason that the only person who could affirm or deny the suspicion of syphilis is not in a state to give reliable testimony, but something over ten years of pretty active service in this field has not given me any data to confirm Professor Wood's theory. I have been in the habit of using during that time doses of the iodide, ranging from thirty to three hundred grains daily, in syphilitic and non-syphilitic cases, but I feel as confident as I do of any conclusion drawn from experience, that the tolerance in syphilitic cases was not sufficiently marked, as compared with non-syphilitics, to offer any grounds upon which to base a theory or make a diagnosis.

"I do not know of any reason for supposing that the tissue-changes from syphilis are alone of such a character as to cause a tolerance of the iodides in the animal economy, if we admit even that such tolerance may be produced by disease. An examination of the facts would seem to indicate the contrary, as well as when we argue from theoretical grounds alone. No disease yields so readily to medicine as late syphilis does to the iodide of potassium, and yet Professor Wood asks us to believe that the very drug which has such a power over the disease is rendered by that disease innocuous to the body in which the disease resides.

"I do not see that anything is to be gained by Professor Wood's suggestion, as much as I am loath to differ with so able and experienced an observer.

"If syphilis cannot be diagnosed by other means, why is it necessary to call the case syphilitic because a drug successfully employed in the treatment of syphilis is tolerated, or even brings about a recovery? If mere toleration is all that is observed, nothing is gained, surely; and if recovery takes place, what advantage is there in knowing that 'for all practical purposes' the case was syphilitic?

"If the iodide of potassium had no remedial power except in syphilis, a diagnosis might be safely made after successful treatment; but mere tolerance of the drug would give no safe and proper ground for judgment. It seems to me that nothing is to be gained, and some risk at least of injustice to the patient is incurred by such reasoning.

"Very sincerely yours,

"EDWARD N. BRUSH,

"*Assistant Physician Pennsylvania Hospital for the Insane.*"

"January 30, 1889.

"DEAR DR. WHITE,—The keynote to the discussion between yourself and Dr. Wood seems to me to hinge upon the latter's statement that 'In all cases of doubtful diagnosis (of cerebral syphilis) the so-called therapeutic

test should be employed, and if sixty grains of iodide of potassium per day fail to produce iodism, for all practical purposes the person may be considered to be a syphilitic.'

"Leaving out of the question, for the sake of brevity, the intimate changes induced in the system by the iodides and the nature of the syphilitic person, I will briefly give you the result of my observation and study of the action or rather behavior of these drugs.

"In the first place, I must protest against the current idea of confounding idiosyncrasy with intolerance. Very many persons have an idiosyncrasy against the iodides, but in very few is there an actual intolerance. This is particularly fortunate for them and for us.

"As to the action of the iodides in non-syphilitics my experience has convinced me that these salts are well borne by very many patients of both sexes and of all ages. In some, however, they produce such lesions and affections as dermal inflammations, coryza and lachrymation, œdematous swelling of the parts supplied by the fifth nerve, cerebral hyperæmia, mental and physical depression, and gastro-intestinal irritation.

"Of these cases, it may be said,—

"1. That they may be persistent even when all possible means of amelioration have failed.

"2. That in many instances, if the drug is pushed, notwithstanding perhaps some discomfort to the patient, these unpleasant and sometimes alarming phenomena will cease and the drug will be not in any way harmful and actually beneficial.

"3. In some of these cases we, as they say, 'weaken' too quickly and give up the drug, whereas, if with proper moral courage (the urgent necessity of course existing) we increase the dose and go ahead, toleration is established. In many cases in my experience *small initial doses* have produced mischief, while the succeeding larger ones have been very acceptable.

"4. In many cases, abstinence from liquors, alcoholic and fermented, care as to the simplicity and easy digestibility of the food, requisite medication for the stomach, and a general improvement of the alimentary canal will be followed by a proper acceptance of the drug, perhaps with some preliminary skirmishing.

"5. I have seen several cases in which the iodides were well borne, previous to the onset of pathological changes in the kidneys, and after the evolution of the latter they were more or less toxic in their action, sometimes so much so that their administration was of necessity suspended.

"6. I call to mind three cases in which, while the patients were high livers and deep drinkers (one exclusively of champagne), the iodides were not borne without great discomfort, but when they discarded these irritants and stimulants, the iodides produced no discomfort (cerebral, dermal, nasal).

"7. I have seen cases in which an intolerance of the iodide of potassium lasted twenty years, and at both ends of that period produced a characteristic bullous eruption.

"My experience has not convinced me that syphilis gives a man any immunity, partial or complete, to the toxic action of the iodides. Indeed in general the experience I have already detailed with virgin subjects applies to syphilitic individuals. The secret of very many cases rests in the fact that dire necessity compels the continuance of these drugs, and the so called intolerance is forgotten.

"In syphilitics as in healthy subjects an intolerance of to-day may be replaced by a condition of assimilation a month, a year, or several years hence.

"I call to mind the case of a man who had gummatous infiltration into the soft palate, and seemed intolerant of the iodide of potassium. I pushed it, however, and his intolerance ceased before the infiltration was absorbed. Four years later (after a life of great indulgence) he had syphilitic pachymeningitis, took heroic doses of the iodide, showed no intolerance, and got well.

"I strongly suspect that many cases which have led Dr. Wood to formulate his therapeutic test are like the one just mentioned, and that a previous course of the iodides has conquered the intolerance which may have existed in them.

"In this connection I would call your attention to the case of 'Dermatitis of Iodic Origin,' reported and figured by me in the *New York Medical Journal*, November 3, 1888. The patient, three years syphilitic, suffered from nocturnal pain in the head, not localized but more severe in the forehead and eyes. For six days he took twenty grains of the iodide thrice daily, for six days longer he took twenty-five grains, then the dose was increased to forty grains, with the effect of producing in fifteen hours one of the most severe forms of iodic eruption known. The necessity, however, existed for still larger doses, and he got them without any intermission. The dermal lesions ceased and he lost his pain in the head. He had enlarged spleen but healthy kidneys.

"In conclusion, after a careful study of your

papers and of Dr. Wood's rejoinder, leaving out of consideration all hypotheses and judging from the facts of observation and study as far as I am able to do so, I must say that I think that your three postulates stated upon page 8 of your pamphlet are sound.

"Very truly yours,

"R. W. TAYLOR,

"*Professor of Venereal and Skin Diseases in the University of Vermont.*"

"16 WEST THIRTY-SECOND STREET,
"NEW YORK CITY.

"DEAR DOCTOR,—With regard to the question upon which you have done me the honor of asking an opinion, as to the diagnostic value of the tolerance of the iodides in syphilis, I believe that the idea, which is still quite prevalent, that if a given obscure lesion disappears under the use of the potassium iodide, or if the iodide is tolerated in large doses, that therefore it must be syphilis, is a fallacious and misleading one. Too much stress has, I think, been laid upon the touchstone theory of the action of medicines, and in none more than in the results obtained from the use of mercury and iodide of potassium.

"Putting to one side for the moment those cases of syphilis in which organic lesions have occurred and where no good has ensued from the administration of large doses of the potassium iodide, although they have been well borne, there are other cases where the same tolerance of the drug is shown in which the disease is not syphilitic, as witness cases of cyclitis, gouty affection of the joints, and the like. Here the iodide often acts like a charm; and although the question may be raised whether the lesions are not really due to syphilis, notwithstanding that no history or evidence of the disease exists, still I think that does not invalidate the correctness of the diagnosis, and the maxim '*de non apparentibus*' may be fairly invoked in support. Again, if the effect of the iodide in syphilis is to be taken as a guide or index of the nature of the affection under consideration, how is it that in the earlier stages of syphilis the iodide is not so well borne and is more liable to produce disturbance than when given in the later phases of the disease? And yet such has been my experience, and no doubt the experience of others.

"Taking the reverse of the proposition: idiosyncrasy often acts in such a manner as to preclude the administration of the potassium iodide, and still the disease remains syphilis; yet were the rule—that if the iodide is not tolerated therefore the disease cannot be syphilis—acted upon, the surgeon would be led into error, to the detriment of his own reputation and his patient's welfare.

"In Dr. Wood's paper I see that he also has found that patients showing the early stages of syphilis do not stand the iodide as well as those who are in the advanced stages of the disease, and in so far I agree with him, but there are one or two points upon which I should take issue with him. On the 12th page he writes: 'The production of iodism by small doses of the drug is, however, evidence (not proof) that the patient is not suffering from syphilitic disorder, and in any given case renders it probable that the symptoms are not the result of syphilitic infection.' My opinion is that such a condition would be neither evidence nor proof that the disease was not syphilis, for I have found that small doses of the iodide, and that in syphilitic persons, will more readily produce the toxic symptoms of the drug than when it is administered in large doses,—and by small doses I mean 20 grains or under, and by large ones above that amount. To my mind, therefore, the small dose is no test.

"On page 15 he writes of the slowness in action of the iodides. My own experience points in the opposite direction, for, if I desire to get a quick result in checking the extension of an ulceration or the increase of a gummous growth, my first weapon is large and liberal doses of the potassium iodide; but one point, which should be borne in mind as regards the good to be obtained from the use of the iodide, is that its action is limited, and it is not, to my mind, a curative agent in the treatment of syphilis. It is an adjuvant to mercury and an admirable assistant in many cases, but it is not the right bower, hence what Dr. Wood observed in the autopsy of his epileptic patient—recorded on pages 15 and 16—is what should be expected. He is quite right when he goes on to say that 'more and more has it come to be with me a favorite rule of action in cerebral syphilis, without evidences of cachexia or a distinct history of mercurialization, to begin the treatment with mercury,' but I think he is not so much in the right when he goes on to say 'in such doses as to cause slight salivation, and to maintain this impression for some days or weeks *pro re nata*.'

"In no cases of syphilis do I believe it necessary to induce ptyalism during the course

of treatment; indeed, I am inclined to regard it as a disadvantage rather than a benefit. Nor do I, as does Dr. Wood, consider it necessary to administer the iodide after a mercurial course in order to produce elimination of the mineral, for I believe the mercury is promptly eliminated by itself if the drug be properly administered.

"And now, dear doctor, I think I have stated my opinion on this interesting subject, as you have asked me to do; and with many apologies for my delay in replying, believe me,

"Very sincerely yours,

"F. R. STURGIS,

"*Professor of Venereal Diseases in the Medical Department of the University of the City of New York.*"

"2 WOODSIDE TERRACE, GLASGOW.

"MY DEAR SIR,—In reply to your letter of 6th January, I have to say that, in my experience, the occurrence of the phenomena of iodism depends upon idiosyncrasy. I have often given the iodide of potassium in very large doses, even to the extent of two ounces daily, to non-syphilitic subjects (*e.g.*, to patients suffering from undoubted non-syphilitic psoriasis, and from aneurism without syphilitic antecedents), without any unpleasant symptoms, and I believe that iodism results much more frequently from small than from large doses.

"I have frequently observed iodism set in, and generally soon after the medicine was commenced, in syphilitic subjects, both in the secondary and tertiary stages, and have found it occur just as frequently in syphilitic as in non-syphilitic persons. So that I entirely agree with you in placing no confidence in the diagnostic value of the tolerance of the iodides in syphilis.

"Yours, sincerely,

"T. MCCALL ANDERSON,

"*Professor of Clinical Medicine in the University of Glasgow.*

"To J. WILLIAM WHITE, M.D.
"January 18, 1889."

"NEW YORK, January 2, 1889.

"DEAR DOCTOR,—I consider Professor Wood's statement that 'if sixty grains of iodide of potassium per day fail to produce iodism, for all practical purposes the person may be considered to be a syphilitic,' to be a dangerous generalization, likely to lead to looseness in diagnostic methods, to say nothing of the moral aspect of the question.

"I have neither time nor inclination to discuss the subject here, but my belief is that the whole matter of the tolerance of iodides is personal with the individual, that this personality is not modified by syphilis, but it may and does change in some individuals, and that too without the intervention of syphilis.

"This is my general view, which I could sustain at length, but that I am overborne at this moment with a multiplicity of duties and responsibilities.

"Yours truly,

"EDWARD L. KEYES,

"*Professor of Genito-Urinary Surgery and Syphilology in Bellevue Hospital Medical College.*"

"BUFFALO, January 1, 1889.

"MY DEAR DOCTOR,—I don't think my views on the subject of your recent paper are worth perusal, but I am inclined quite strongly to the opinion that iodism is a matter of idiosyncrasy vastly more than of dose, particularly those severer forms which prevent its administration. Jullien's theory appears to me untenable.

"I see very few cases of purely nervous syphilis, but I should hardly act upon Professor Wood's theorem for a working basis. The cases to which he refers are the uncertain cases, where diagnosis is either difficult or impossible. The disappearance of the symptoms under the iodides (one drachm per diem) might be presumptive evidence in favor of their specific character, but I should be loath to accept the statement that one drachm per day would cause iodism unless the case were syphilitic,—providing that by his expression 'iodism' he meant something more pronounced than a few pustules on the face, etc. And even in case he includes under 'iodism' the slightest manifestation I should consider it too sweeping a statement.

"Perhaps we all view the matter from the specialist's stand-point, but I am inclined to coincide with the views expressed by yourself. I have in mind a few cases which give me a

present reason for my convictions, and yet not one of those I now call to mind was under personal observation for a sufficient length of time to justify any other use of it than to put it in a group with others and try to thus make it of some aid.

"As one who deals with the surgical aspect of these cases oftener than with their uncomplicated drug-treatment, I may be excused from allowing your complimentary request for my opinion to put me in a position where I might find myself without adequate means of defence.

"Sincerely yours,

"ROSWELL PARK,

"*Professor of Surgery in the Medical Department of the University of Buffalo.*"

"DEAR DOCTOR WHITE,—The point at issue between you and Dr. H. C. Wood I take to be, Whether tolerance of the iodides in a given case is, *quoad* syphilis, of diagnostic value? I confess to have shared the widely-spread belief in the affirmative proposition, but in looking over my notes I find no data from which I can glean a positive answer one way or the other. I find two groups of cases in which I have given the iodides in large doses, —cerebral syphilis and aneurism. Certainly patients with the former affection bear, as a rule, very large doses, which may be kept up for months, but I call to mind two cases at least in which this tolerance existed, and in which the lesions proved, post mortem, not to have been specific. On the other hand, I have seen instances of undoubted syphilis in which intolerance of the iodides was a striking and persistent feature.

"Some years ago I was in the habit of giving large doses of iodide of potassium in cases of aneurism, and I have frequently referred to the apparent tolerance of the drug presented by these patients, yet the case which bore for the longest period—three months—the largest doses had not had syphilis.

"I am inclined to regard intolerance as a matter of idiosyncrasy, and should say that, while a large proportion of all patients bear the iodides in full doses for long periods, a small number cannot take even minimum doses without unpleasant symptoms. The 'general impression' which I have had as to the tolerance of the iodides in syphilis has no doubt resulted from the fact that I have given large doses for long intervals in this disease, and in aneurism, so often associated with syphilis. I willingly acknowledge this impression to be worthless in the absence of control-observations of equal number in other diseases.

"Yours sincerely,

"WM. OSLER,

"*Professor of Clinical Medicine, University of Pennsylvania.*"

The extraordinary avalanche launched upon me by my friend Dr. White, I confess at first took my breath; but when I remembered that the root-diggers of the Brazilian forests, finding that a certain drug was being exterminated, quietly substituted the root of a plant apparently selected at random, and that the urino-genital surgeons never perceived any change in the action of the remedy, but continued using the new root under the old name, I was enabled to sufficiently recall myself to say a few words in reply.

I have been very much surprised that some of the writers have thought that I was advancing a novel doctrine or belief. My opinion is exactly as novel as it would be if I said that mercury was useful in inflammation, or digitalis in heart-disease. Glancing over a few of the late lectures of Professor Gowers ought to convince any one that the words "therapeutic test" have become a classical technical expression.

Many of the gentlemen who have written are syphilographers. Such specialists see cases in which the person recognizes that he is suffering from syphilis. In the majority of cases of nerve- and visceral-syphilis, the patient has no suspicion that he is suffering from syphilis, and does not consult the syphilographer, so that the experience of the latter is drawn from general syphilis, and not especially from the gummatous disease which influences the nervous system or the internal viscera. The practical importance of this must be apparent when it is recollected that the idea which I promulgated, that it was the latter stages of syphilis in which the relations of the system to the iodides are changed, receives distinct and positive confirmation at the hands of one or two of my opponents.

To attempt to answer the various letters in detail would be folly,—life is too short; but as

several of the correspondents, and especially my friend Dr. Chapin, have failed to understand my original paper, I propose to formulate my views in a few sentences.

First.—There is no proof that iodide of potassium affects directly, or has direct interference with, the syphilitic virus.

Second.—In the state of the system which occurs in advanced syphilis, there is habitually such a change in the human organism, that iodide of potassium is usually tolerated much more freely than in health.

Third.—That the statement made in paragraph number two is simply a general statement, useful in the prognosis and treatment of nervous or advanced syphilis,—not without exceptions, not a law, but a rule or coincidence of sufficiently wide application to be of practical value in prognosing and treating disease.

I have seen cases over and over again, in public clinics and private consultation, in which the therapeutic test has served me faithfully, and have very many times reached the desired goal, and propose, therefore, to continue to use the old, time-worn probe, believing that a physician is a fool who tells a patient or his friends that the case is syphilis because the therapeutic test is responded to; but that the physician who, in making his decision, fails to give due weight to the test, will fail to succeed in many obscure nervous cases, where he ought to have made out the true nature of the disease.

My opinion in regard to the relations between the iodide of potassium and nerve-syphilis is founded upon a very wide experience, but I must further state that it is consonant with general well-known therapeutic rules or laws. Dr. Brush says, "No disease yields so readily to medicine as late syphilis does to the iodide of potassium, and yet Professor Wood asks us to believe that the very drug which has such a power over the disease is rendered by that disease innocuous to the body in which the disease resides,"—apparently, with the thought that my asking any one to believe such a thing is absurd. If Dr. Brush had been a general practitioner, instead of a specialist in insanity, he would have no doubt known that it is a general law, rule, or coincidence that, when there is a systematic disorder which is curable or removable by a drug, the systematic disorder renders the system comparatively insusceptible to the action of this drug. In severe malarial poisoning, doses of quinine which would overwhelm the nerve-centres of the ordinary individual fail to produce tinnitus aurium; in snake-bite poisoning, pints of whiskey can be taken without intoxication; in syphilitic subjects, mercury is usually required in much larger quantities to cause ptyalism than in the ordinary individual; in delirium tremens enormous doses of narcotics are well borne; in low fevers, much more alcohol is destroyed and much smaller effects are produced by alcohol than in health. Even in local disease the same rule holds to some extent: the irritation of a peritonitis causes the brain not to feel the ordinary dose of opium; a constipation buoys up the man against a purgative; a violent pain or internal irritation prevents the action of a counter-irritant; and so on to the end of the chapter. So far, then, from my belief being an absurdity, the possession of the power by the drug over syphilis is a strong reason for believing that the disease renders the system insusceptible to the drug.

There are certain things in the letter of Dr. Sturgis, outside of the matter which is especially under discussion, of such great practical importance that I feel it a duty to say a word in regard to them.

The problem whether the metal is or is not all eliminated from the system during a long mercurial course is one which is purely chemical, and is capable of an absolute chemical solution as complete as that of the toxicologist in a case of poisoning by arsenic. This solution has apparently been made by various chemists, and from present evidence it *appears* to be positively proven that the mercury is not eliminated from the system: I must hold, therefore, that the man who lets his clinical surmises guide him in the face of direct positive chemical evidence is without excuse. To my thinking, it is the absolute duty of a person who holds the opinions of Dr. Sturgis to make or to have made a series of chemical experiments which shall disprove those made by other chemists, and that, until such chemical proof is furnished, the practitioner of medicine has no moral right to give a mercurial course without following it by the iodide of potassium.

H. C. Wood.

PARIS.

(From our Special Correspondent.)

The Pasteur Institute, as the readers of the THERAPEUTIC GAZETTE probably know, is intended not only for the treatment of hydrophobia cases, but also for the study of microbian diseases in general, voluntary donations from all parts of the world having munificently endowed the establishment. Two of its chemists, Drs. Roux and Yersin, after experimenting with the *diphtheria bacillus* observed by Klebs in 1883, and studied by Lœffler in 1888, have made an important contribution to medical sciences, which has been the event of the month. The chief point of their discovery is, that the microbe is not one that, like most bacilli hitherto observed, causes sickness and death through its multiplying in the economy or some particular organ. The diphtheria bacillus, they have proved, has a fixed habitat, some mucous surface where it develops and always remains; and there it secretes a specific poison, causing all the various symptoms of the disease. Whether the agent is an alkaloid, a diastase, or any other compound has not been ascertained, but Drs. Roux and Yersin have demonstrated beyond possible doubt that the secretion acts chemically, as an ordinary drug or poison might. So, to a certain extent, the bacillus comes within the domain of therapeutics. To begin with the demonstration, many post-mortems on man and animals showed the bacillus to be invariably absent from all parts, except those affected and covered with the characteristic membrane. But this is only a negative proof. The positive has been brought to light in the clearest manner, as will appear hereafter. It was already known that inoculation will produce in pigeons, rabbits, and guinea-pigs an affection closely similar to diphtheria. The first step has been taken successfully to prove that the disease is not only similar but the identical one, with all the symptoms of diphtheria, paralysis included. The inoculations were performed from man to animals, and from animal to animal, by scratching, with a platinum-needle holding the contagious substance, some mucous surface, such as the throat or the vagina. The capital point is that on cultivating the bacillus of human or animal origin, in appropriate media, bouillons will be obtained, which, being freed from all micro-organisms by filtering through unglazed porcelain, will cause, when injected into the veins of animals, all the symptoms of diphtheria during life, and its characteristic lesions observable after death. The quantity of bouillon necessary varies with the kind of animal and the virulence of the preparation. But in no case is there any diphtheria-membrane ever produced on the mucous surfaces. The next thing now is to find a remedy. The nature of the disease being at present better understood than it ever was, hope may be entertained that the cure will be discovered before long. But one good result at all events has so far been secured,—namely, a knowledge of the importance to be attached to local antiseptic applications. The treatment was already sanctioned by practical experience, but now we understand the why and wherefore.

There are degrees in drunkenness, but there is also it seems a sort of gradation in the kinds of stimulants, a scale extending with the progress of science. Alcoholic beverages, the time-honored intoxicants, have of late been supplemented with ether, chloroform, and chloral; and, lastly, with morphine and cocaine injections. And it is seldom a man takes to the more potent drugs before he has abused alcohol. He generally begins with the glass and ends with the syringe, so to speak. From observations made by Drs. Magnan and Chouppe, the *cocainism cases* that came before them had all previously been *morphiomaniacs*. So that we have here the natural scale,—ascending or descending, as you like,—alcohol, morphine, cocaine. The description of the symptoms is instructive. Dr. Magnan had to deal with three chronic cases presenting some notable peculiarities common to all,—namely, a perversion of general sensibility, a sort of hallucination which causes the patient to feel as if he had some foreign body under the skin. One would scrape his tongue to squeeze out little black maggots, perceived by no one but himself, and look into the abscesses to dig out cholera microbes. Another, a pharmacist, would tear up his skin, and with his nails or the point of a pin bring up from underneath quantities of microbes; and the third patient, a physician, would persist in searching under his skin for crystals of cocaine. To which a case may be added observed by Dr. Jennings, wherein the patient was also looking for maggots under his skin. In the three instances now alluded to, the disorders affecting sensibility were the first to become apparent; the hallucinations of sight, hearing, and smell developed only subsequently, and never were so violent as those due to alcohol or absinthe delirium. Analgesia was but slightly noticeable, and only on some points. And, finally, the two first pa-

tients had epileptic attacks, but the third only cramps, which symptoms all disappeared with the suppression of cocaine. Dr. Chouppe, on his side, had also a quota of three cocaine patients, and all were inveterate morphiomaniacs, and, like the three others, began with very large doses of cocaine. But here the similarity ends, none of the second lot having kept on long enough to be exposed to the accidents of chronic cocainism. As figures have an eloquence of their own, it is instructive to know that one of the patients began with 50 centigrammes (8 grains) of cocaine muriate injected in one dose. The two others, less heroic, started with 25 and 20 centigrammes, respectively, far from a despicable quantity. The patients bore well the doses, which would have poisoned, perhaps killed, any one but a morphiomaniac.

The influence of *chronic lead-poisoning* on pregnant women and the fœtus was brought to light in 1860 and 1861 by Dr. Constantin Paul. A new instance, interesting on account of the thoroughness of the observation, has just been related to Drs. Hermann-Legrand and Winter of this city. It occurred as follows: The patient, a woman, aged 27, worked as a compositor in a printing establishment. She had six times been to the hospital before for lead-colics. Five previous pregnancies resulted four times in abortion, and once in a full-term child, who died of convulsions seven months afterwards. On the present occasion she entered Dr. Budin's service at the Charité Hospital, *enceinte* for the sixth time, and seven and a half months pregnant. She recently suffered again from lead-colics, and presented the usual symptoms of the poisoning. Three days after her reception she gave premature birth to a child, but recovered without accident. The husband, it should be added, is a compositor, also affected now and then with the lead-symptoms, but never to any serious extent; no syphilitic taint was observed in him, but a little alcoholism. The child, a boy, was puny, ashy-pale, weighed ten hundred and twenty grammes (about thirty-five ounces avoirdupois), and died after fifteen days. At the autopsy, besides the peculiar appearance and dwarfish size of the kidneys, and a cirrhosis of the liver, the remarkable point was the presence of considerable quantities of lead in the hepatic parenchyma. None was found in the kidneys; they had evidently been impeded in their development, but held no lead. This observation demonstrated that one of the most insoluble of poisons may be transmitted from mother to fœtus, and cause in the child's organs lesions sufficient to render them unfit to carry on life. This much so far may readily be taken for granted, as it is supported by clear evidence. But Drs. Hermann-Legrand and Winter go further, on ground scarcely solid enough. They think that besides hereditary saturnism, there exists quite a series of similar poisonings not only mineral but also organic, and even bacteriological in their origin. For the present it seems safer for us to stop at lead, and, so far as other hereditary poisonings are concerned, to wait for further developments.

A case of no little concern to surgeons has just been decided by the Paris courts. The suit had reference to the mooted question,—namely, the *responsibility of surgeons*, and was as follows: A retired officer, suffering from angular anchylosis of the knee, went for treatment to the Val-de-Grâce Military Hospital, and placed himself under the care of Dr. Poncet, an army surgeon of no mean repute. It may here be explained that the hospital in question is at the same time the seat of the finishing school for young military surgeons and pharmacists. Dr. Poncet advised the straightening of the leg by means of a certain orthopædic apparatus well-known here. Somehow the treatment and its results failed to satisfy the patient, who sued the surgeon for malpractice, and prayed for ten thousand dollars' damages. The issue was well fought on both sides, Dr. Poncet, as an army officer, having the support of the war department's attorney and lawyers. The judge held that, although courts have an undeniable right to examine into facts to decide whether a physician has been imprudent or neglected due precautions, it is not within their province to enter into scientific questions or appreciations of medical practice; that defendant, under advice of two physicians, asked to be admitted to the hospital with the intention of being operated on; that Dr. Poncet only proceeded to the operation after one month's observation of the case, and calling in two other surgeons, with whom he consulted; that the presence during the operation of the apparatus-maker was a fact in favor of defendant, showing carefulness instead of imprudence, as charged by plaintiff; and, finally, that the court cannot appreciate the skill of a surgeon or decide upon the use of a particular instrument. In consequence the court found for defendant, and ordered plaintiff to pay all costs. If the doctor was well defended, the other side was equally well represented, the plaintiff being a man of wealth

and influence. Besides, a thorough preliminary inquiry had been ordered at the request of the plaintiff. The judicial decision has all the more weight that the suit was more fiercely contested.

Dr. Brown-Séquard warmly recommends *carbonic acid as an analgesic*, acting through its effect on the mucous surface of the larynx. It is quite harmless, provided the gas is kept from reaching the lungs, where it might be absorbed. To effect this, carbonic acid should only be inhaled near the end of each inspiration, so that the gas will reach no farther than the larynx and the upper part of the bronchial tubes, whence it will be expelled by the expiration. It is well to gradually accustom the parts to the presence of the gas, otherwise painful dyspnœa might ensue. And, lastly, the inhalations must be kept up for half an hour. With the foregoing precautions it has been possible to produce an analgesis lasting thirty hours, and so complete as to deaden the aching of a very painful lumbago, and to allow the painless massage of a sprain. The administration of carbonic acid, according to Dr. Brown Séquard's process, seems rather long and laborious when compared with other analgesics, but its complete safety may by some be considered ample compensation.

A useful lesson on the *benefits of sanitation* may be drawn from an official report just published by Dr. Lagneau on the death-rate of French soldiers in the colonies. In civil life the rate for men from twenty to twenty-three is at home eight to ten per mille, and nine to eleven in military service. In Algeria, from 1837 to 1848, the rate was seventy-seven, and has now come down to between eleven and twelve, or very nearly the same as at home. In Tunisia the proportion has, under proper sanitary care, come from sixty down to twelve. The Oceanican possessions are the healthiest, eight to nine being the rate for Tahiti and New Caledonia. The French West Indies have improved from ninety-one to twenty per thousand, which is also the present rate for Cochin China, after having reached one hundred and fifteen at the beginning of the occupation. That "precious" Tonquin works the other way; from 1882 to 1885 the rate had come down to forty, but since then, under the influence of cholera, it has reached ninety-six. At La Réunion Island the usual death-rate is twenty-nine per thousand, but when it became the base of operations for the Madagascar expeditions, and received the sick, the proportion rose to seventy and one hundred and thirteen. And, finally, the Senegal colony is the worst of all. The climate is so deadly that the average death-rate from 1832 to 1837 was one hundred and forty-eight per mille; but even there sanitary care had brought down the proportion to seventy-three, or about one-half.

PARIS, February 15, 1889.

Notes and Queries.

A NEW REMEDY FOR CHOLERA.

LOEWENTHAL (*Acad. des Sciences*, Session December, 1888) has concluded a course of experiments undertaken to find an antidote to the virus of cholera. This toxic principle is now, according to the newest pathology, regarded as the product of Koch's cholera bacillus,—a ptomaine, in fact, which is destroyed by cultivation in artificial nutrient media. Loewenthal has found that a pure culture of these cholera bacilli in peptonized broth, previously sterilized, is absolutely inoffensive to animals—as white mice—naturally susceptible to the cholera poison; the bacilli ceasing to produce the noxious ptomaine.

The first aim of Loewenthal's experiments was to render to the cholera bacillus, by a process of the laboratory, the toxic property which it possesses when fresh, but which is lost on cultivation. After many fruitless essays, he believes that he has succeeded with a paste which contains pancreatin, and the composition of which is as follows: fresh pork (muscle), hashed, 500 grammes; pancreas of hog, hashed, 200 grammes; bean flour, 100 grammes; peptone, 15 grammes; grape-sugar, 10 grammes; common salt, 5 grammes. These substances, mixed with water or milk, give a soft paste, almost liquid, which is rendered alkaline by a little potash, and then sterilized by hot steam. The cholera bacilli, which by culture have lost their pathogenic properties, are allowed to breed in this artificial paste. They immediately secrete their virulent ptomaine, which, when inoculated in mice, either kills these animals or makes them intensely sick.

By varying the elements of his culture mixture, Loewenthal finally satisfied himself that it is the pancreatic juice which, in presence of albuminoid and peptonized substances, determines the pathogenic or poison-secreting action of the bacillus. All the other culture

media (peptone-gelatine, agar-agar, bouillon) assure the development of the bacillus, but no toxic matter is produced.

This peculiar action of the pancreatic juice being understood, we have, says Loewenthal, an explanation of the phenomena of cholera in man. The bacilli, after being ingested, escape the stomach, and entering the intestine, produce there, with the help of the pancreatic juice, the same toxic matter which is produced in the pancreatic paste, the latter being a coarse imitation of the contents of the duodenum ; this toxic matter is absorbed and the restoration or death of the patient depends on the quantity of poison absorbed and the resistance of the organism. This experimental fact is in harmony with the anatomo-pathological fact that the bacilli of cholera remain always confined to the intestine, as well as with the "fulminant cases," and the experiments of Nicoti and Reitsch, and those of Koch on animals.

This point being once determined, Loewenthal asked himself if there might not be some substance inoffensive to man which, introduced medicinally, would prevent the development of the cholera poison in the intestine. To determine this, he first experimented with his pancreatic paste, trying various antiseptic agents which he thought might prevent the active functional operations of the bacilli and the genesis of the toxic ptomaine. Any agent, he reasoned, which can accomplish this *out of the body*, might be relied upon to do the same *within the body*, and thus become a specific (preventive and curative) remedy for cholera.

This remedy Dr. Loewenthal announces that he has found in salol, the salicylate of phenol, discovered in 1886 by Nencki, of Berne. This powerful antiseptic is decomposed in the organism by the pancreatic juice, the same agent which renders toxic the cultures of the cholera bacillus in the pancreatic paste. A multitude of experiments have assured him that salol in presence of fresh pancreatic juice is invariably fatal to the cholera bacilli in his laboratory culture-tubes ; and he has determined the quantity which is sure to effectually sterilize his cultures, namely, two grammes of salol to every ten grammes of the paste ; a smaller dose, however (as ten centigrammes), renders the bacilli inactive.

It is known that salol can be taken in pretty large doses (as much as ten to fifteen grammes a day) by man with comparative impunity.

It must be added that the above interesting laboratory experiments, conclusive as they seem to be to their author, who has full faith that he has now found a sure specific for cholera, still lack clinical confirmation, as well as that confirmation which comes from a series of carefully conducted experiments on animals.—*Boston Medical and Surgical Journal*, February 7, 1889.

COLDS AND CLOTHING.

The teaching of modern science and of ancient custom goes to show that heat-production within the body has much to do with the tissue-changes concerned in muscular activity and with healthy digestion. It is conserved by warm and moderate, wasted in evaporation by excessive, clothing. Finally, by a simple nervous reaction, it is increased after the contact of external cold. It follows from these observations that, if we be so clad with comfortable underclothing that surface perspiration is not formed in excess and is rapidly removed, one great cause of chill—sudden evaporation—is done away with. Outer cold, then, provided it is not too severe, only touches, as it were, the spring of the heat-making metabolism, and, exciting an elastic rebound in the chain of vaso-motor fibres, awakens that oxidative action by which every tissue is made to yield its share of heat to the body. This bracing influence is lost wholly or partly to those who are too heavily clothed, and in its place we may have a dangerous excess of surface heat. It is for this reason that we have before protested, as we now do, against the indiscriminate use of the thick and heavy overcoat. We would rather see men in fairly robust condition, especially if young, clad warmly next the skin, and wearing either a light top-coat or none at all. There can be no doubt that the habitual use of great-coats is indirectly accountable for the chills which they are intended to prevent. Were the overcoat worn continually it might attain its object. Its intermittent use, even when ample underclothing is worn, affords no solid guarantee of safety, but rather the reverse. The man of sedentary habits has especial need to remember this. He emerges daily from a warm breakfast-room clothed in his ordinary winter garments, with probably woollen underwear, and over all the heavy ulster or top-coat. After a short walk he finds that the sense of warmth he began with is more than maintained. He arrives at his office or place of business, and off goes the overcoat, though the air of the newly-opened room is as cold as that without, and draughty

in addition. During the day perhaps he travels to and from adjacent business houses wearing only his house-clothing. The overcoat is laid aside till closing time reminds him of the journey home. The frequent result is that somehow between the hours of his departure and return he is chilled. No doubt he would run as great a risk if, lightly clad, he were to face the rigor of a winter day. In this case, however, exercise and habit might do much to develop the power of endurance, and there would, at all events, be less danger of sudden cold acting upon a freely perspiring surface. Woollen underclothing represents a state of healthy comfort intermediate between these extremes, and more resistant to chill than either. In commending its use, however, we do not assert that the influence of age and constitution is to be overlooked. Youth can oppose a power of resistance to depressing agencies which does not reside in the worn-out nerve-centres of a riper age. Similarly, that elastic reaction which characterizes the nervous and sanguine types is not to be looked for in the lax tissues of the lymphatic. The weaker physique naturally calls for fuller protection than the stronger; and any rule requiring the disuse of the overcoat should allow of reasonable exceptions in favor of the old and constitutionally feeble. Unusual severity of weather, especially if associated with night air and the loss of sleep which this implies, is another condition which might well constitute an exception. In such a case we are compelled to add some form of overcoat to the ordinary amount of clothing. Some parts of the body—for example, the chest, throat, and feet—are certainly more susceptible to cold than others. As a useful safeguard, cold or tepid bathing of such parts is in merited favor. The custom so common with many persons, especially women, of walking out in thin-soled boots often plays an important part in catching cold. The progress of time and of rational thought may be expected to bring a more comfortable arrangement by clothing the foot in woollen hosiery and a stouter boot.—*The Lancet*, January 26, 1889.

OPHTHALMIA NEONATORUM.

Dr. Y. Sant'Anna, in a paper read before the Society of Medicine and Surgery of Rio de Janeiro on the subject of ophthalmia neonatorum, expressed a very strong opinion that this affection never occurs except under circumstances where gonococci are present in the maternal passages. He prefers Crédé's prophylactic method of dropping a solution of nitrate of silver into the eyes as soon as the child is born to Gusserow's plan of antiseptic vaginal injections. He would employ the instillations in all cases where the woman is suspected of being a loose character, in multiparæ where the former children have suffered with ophthalmia, where the woman belongs to the very lowest classes, and where the father has had gonorrhœa. In the course of his paper, Dr. Sant'Anna mentioned that in the maternity clinic connected with the Faculty of Medicine in Rio de Janeiro no less than twelve per cent. of the children born were affected with purulent ophthalmia. Notwithstanding his preference for the nitrate of silver prophylactic treatment, he would not use it in all cases indiscriminately, having, as he believed, found that sometimes it was of itself capable of setting up a very undesirable amount of inflammation. As may be supposed, Dr. Sant'Anna's views were pretty severely criticised by some of his auditors, who brought forward cases in which ophthalmia neonatorum had occurred where it was impossible that gonococci should have been present in the maternal passages. Again, it was not generally conceded that the instillation of nitrate of silver could ever produce any undesirable effect.—*The Lancet*, February 2, 1889.

PASTES IN DERMATOLOGY.

Since Lassar introduced into dermatological practice the use of salicylic paste the utility of pastes in irritable conditions of the skin has been abundantly proved. Dr. Gruendler, of Hamburg, has recently made some interesting experiments in Dr. Unna's laboratory on the relative capacity for the absorption of water inherent in various powders which might be used in the preparation of these pastes. He found that carbonate of magnesia had remarkable qualities in absorbing water, and therefore ought to be an excellent ingredient for the formation of a paste. Unfortunately, however, pastes made of a mixture of fat and carbonate of magnesia do not possess the proper consistence. When, therefore, this highly absorbent quality of carbonate of magnesia is desired, it is advisable to combine it with the other powders commonly used. For example, fifty parts of oxide of zinc or starch may be mixed with ten parts of carbonate of magnesia, and the whole rubbed up with fifty parts of fat to form a paste, or as a simple absorbent powder it may be very conveniently

used mixed with oxide of zinc.—*British Medical Journal*, December 15, 1888.

POISONING BY CITRATE OF CAFFEINE.

In the *Lancet* for February 2, 1889, MR. T. GERATY reports the case of a lady suffering from migraine, who, at nine o'clock in the morning, before breakfast, took a dessert-spoonful (equal to about two hundred grains) of pure citrate of caffeine in mistake for the granular effervescing form of the drug. Mr. Geraty saw the patient immediately after the accident, when the only complaint made was as to the nauseating taste. The patient was advised to eat a little porridge; an emetic was then given. A quarter of an hour after the reception of the poison there was semi-consciousness, grave depression, extreme pallor, all the muscles completely relaxed, and a decided inclination to sleep; pulse slow, soft, and very compressible; respiration slow and sighing. The treatment adopted was the $\frac{1}{12}$ of a grain of apomorphine, which acted at once and very completely, small quantities of brandy, the recumbent position, and warmth. There was well-marked rigor about an hour after the caffeine was taken. Consciousness was recovered about an hour and a half after emesis, and the faintness gradually passed off.

KAVAINE.

This name has been given by M. LAVAILLE to an alkaloid extracted by him from the kava-kava by the following method, which he contributes to *L'Union Pharmaceutique*: 1000 parts of kava-root, preferably pulverized, are exhausted with alcohol of 60°, and the resultant liquid filtered. The alcohol is recovered from the filtrate by distillation, and the residue evaporated to the consistency of a fluid extract. Add distilled water sufficient to precipitate the resinous matters, filter, and neutralize with ammonia. Agitate the neutralized fluid with ether, and add sulphuric acid drop by drop until the liquid is slightly but distinctly acid. Let stand for twenty-four hours, collect the crystals which will have formed, on a filter, and wash these with frequent changes of alcohol of 95°. The sulphate of kavaine thus obtained is soluble in its own weight of water at 60° F., slightly soluble in alcohol, and insoluble in ether. It appears in prismatic crystals, slightly deliquescent in the atmosphere. The physiological action of the alkaloid is now being tested.—*National Druggist*, February 15, 1889.

ALBUMINURIA AND YELLOW FEVER.

DR. EMILIO MARTINEZ, writing to the *Revista de Ciencias Medicas*, of Havana, on albuminuria in yellow fever, gives it as the result of his experience that the usual recommendation to judge of the gravity of the disease by the amount of albuminuria is untrustworthy, as he has met with cases, of one of which he gives details, where the amount of albuminuria was very considerable, even though the disease ran an exceptionally mild course. In the case in question it appeared as if the poison confined its virulence almost entirely to the kidneys, the liver and other organs being but very slightly affected. A much more important point than the amount of albumen is, in Dr. Martinez's opinion, the amount of urea excreted per diem. This forms a guide to the activity of the oxygenating processes which are going on in the body. If these latter are retarded by the disease the amount of urea is very sensibly diminished. Its quantity thus stands in an inverse ratio to the gravity of the disease, and in this way forms a fairly reliable guide in prognosis.—*The Lancet*, February 2, 1889.

JUNIPER-BERRIES AS A DIURETIC.

According to the Paris correspondent of the *British Medical Journal* for January 12, 1889, DR. GOLDSCHMID, of Fehraltorf, highly praises the inspissated recent juice of common juniper-berries as the best diuretic in children. Attention was drawn to this remedy by Professor J. Vogel, of Dorpat, in his classical handbook on children's diseases. While being most effective, the remedy is exceedingly mild and altogether free from any unpleasant accessory effect. Two or three teaspoonfuls should be given daily, diluted with water and sweetened with sugar. The little patients take it very readily. The author describes a severe case of nephritic dropsy in a girl, aged 7, where the juice rapidly induced a profuse diuresis, a complete and permanent recovery ensuing in a fortnight.

THE WM. F. JENKS PRIZE.

PHILADELPHIA, February 19, 1889.

The first triennial prize of two hundred and fifty dollars under the deed of trust of Mrs. Wm. F. Jenks, has been awarded by the Prize Committee of the College of Physicians

of Philadelphia to John Strahan, M.D., M.Ch., M.A.O. (Royal University, Ireland), 247 North Queen Street, Belfast, Ireland, for the best essay on "The Diagnosis and Treatment of Extra-Uterine Pregnancy."

The writers of the unsuccessful essays can have them returned to any address they may name, by sending it and the motto which distinguished the essay, to the chairman of the Prize Committee, Ellwood Wilson, M.D., College of Physicians, Philadelphia.

The trustees have made arrangements with Messrs. P. Blakiston, Son & Co., 1012 Walnut Street, Philadelphia, for the publication of the successful essay, which will also appear in the *Transactions* of the college for 1890.

JAMES H. HUTCHINSON,
JOHN ASHHURST, JR.,
JAMES V. INGHAM,
Trustees of the Wm. F. Jenks Prize Fund.

LESIONS OF CARBOLIC-ACID POISONING.

In a note of a fatal case of phenol-poisoning, MR. A. G. R. FOULERTON (*Lancet*, January 19, 1889) calls attention to the differences observed, after death, in the mucous membrane from those given by still other writers. He says, "Drs. Guy and Ferrier state: 'The mucous membranes of the mouth, gullet, and œsophagus are pale, corrugated, sodden, and partially detached, and marked by local patches of hyperæmia or capillary hemorrahage.' So also Dr. Stevenson, speaking of carbolic acid, says it 'whitens and shrivels the membranes with which it comes in contact.' Neither of these descriptions would fairly have described the state of things in this case. So far from being sodden, the lining of the stomach had a distinctly stiffened feeling, and its color was a light gray, the œsophageal lining being considerably darker. The mucous membrane of the mouth could alone have been described as pale or white."

EXTRACT OF MALE FERN AS A TÆNIACIDE.

DR. J. O. DE MAN attributes the fact that filix mas has fallen in repute as a vermifuge to the fact that the doses ordinarily employed are by no means large enough. In the *Therapeutische Monatshefte* for January, 1889, he publishes notes of twenty-eight cases in which perfect cure was attained, and in which no relapse occurred, and in only three of which the treatment had to be repeated on account of vomiting. He has given as high as four hundred and fifty grains of the extract in capsules, with satisfactory results. In some cases, however, only one hundred and fifty grains were required, although this was in children of twelve years of age.

THIOL.

Thiol is a new remedy proposed by DR. JACOBSEN as a substitute for ichthyol. It is made by heating the distillate from peat-tar to 215°, with twelve to fifteen per cent. sulphur. By treating this product with strong sulphuric acid the sulpho-acid is produced, which, after purification, is neutralized with ammonia, forming ammonium sulphothiolicum, or simply called thiol. It is soluble in water and in a mixture of alcohol and ether, but not so soluble in either solvent singly. The sodium salt is analogous with the above and contains 12.1 per cent. sulphur. They have the same remedial value as ichthyol.—*American Journal of Pharmacy*, February, 1889.

ANTIPYRIN IN OBSTETRIC PRACTICE.

In the *Northwestern Lancet* for January 15, 1889, DR. TALBOT JONES reports seven cases of labor in which rectal injections of about 30 grains of antipyrin were employed to relieve the suffering from labor-pains. With one exception, he claims that the pain almost wholly ceased, although the uterine contractions continued without alteration or diminution until the birth of the child. He likewise claims that it will produce great relief in obstructive dysmenorrhœa and the after-pains of child-birth. It is to be noted that this is not in accord with the experience of Dr. Huchard as detailed in this issue of the GAZETTE.

UNALTERABLE IODIDE OF POTASSIUM OINTMENT.

A writer in the *Gior. Farm. Trent.* gives the following: Iodide of potassium, white Venice soap, distilled water, of each, 100; glycerin, 600; fresh or benzoinated lard, 300. Dissolve the iodide in the water (using a water-bath) and the soap; add the glycerin, and afterwards the lard. As soon as the lard has melted, stir rapidly and continually, and stand the dish in cold water.—*American Journal of Pharmacy*, February, 1889.